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Photographic Observations of Certain Jovian Phenomena reported by Messrs. Hargreaves, Peek, and Phillips. By W. H. Wright. (Plates 10 and 11.)

In a recent paper, entitled " The Revival of Activity in the Southern Hemisphere of Jupiter," * Messrs. Hargreaves, Peek, and Phillips have given an account of some very remarkable developments that took place in the southern hemisphere of Jupiter during the late summer and fall of 1928. A disturbance, starting in the latitude of the south equatorial belt, and approximately in longitude 128° (System II.), spread rapidly around the planet in both directions along a parallel of latitude, or rather along two close parallels. The latitude of the disturbance was very nearly that of the red spot, so that the latter was caught in the spread of the former, the result being a very considerable alteration of the spot and a modification of the disturbance itself. Another phenomenon observed was the occurrence of an " irradiating white spot," visible as an excrescence on one or the other limb of Jupiter, which was not distinguishable when it passed over the central part of the planet's disc.

An account of some filter photographs, made at the Lick Observatory on Mr. Phillips's suggestion, will perhaps be of interest, not only because they supply photographic records of some of the occurrences described in the paper of which he is a joint author, but also for the reason that they supplement the descriptions of that paper, especially those relating to the " irradiating white spot," and incidentally establish, it is believed, a relationship between that phenomenon and the great disturbance that has already been alluded to. The photographs in question were taken by Dr. J. H. Moore with the collaboration of Mr. J. F. Chappell, Lick Observatory Photographer, and form part of a series of observations

* *M.N.*, **89**, 209, 1928.

undertaken here of Jupiter and other planets by approximately monochromatic light of different spectral regions.* A year or more ago, while I was arranging for a continuance of this work during a long absence, Dr. Moore and Mr. Chappell kindly offered to secure photographs of Jupiter, and, on receipt of a telegraphic request from Mr. Phillips in September of last year, they undertook as complete a programme of observation as other demands on their time, and on the instrumental equipment, permitted. The first photographs were taken on 1928 October 1, and the last on 1929 January 8. The colours used were ultra-violet (approximately 3600 A.), violet (*i.e.* without filter), green, orange, red, and extreme red (approximately 7600 A.). 136 plates, each with 7 images or less, were secured, or about 800 images in all. Dr. Moore and Mr. Chappell have generously placed this material at my disposal and asked me to discuss it.

Alterations in the Red Spot.—Some of the changes in the appearance of the red spot referred to above are illustrated in Plate 10. The colours in which the photographs were taken are indicated above each column. In the first and second rows the spot appears much as it does in photographs taken in recent years,† that is to say, it is dark in violet and ultra-violet light and almost invisible in light of the extreme red. It is longer on October 9 than on October 4, and narrower. The great white irregular area in slightly more northern latitude moved forward (to the left) between these two dates of observation, and the narrowing of the spot seems to be due to the encroachment of that part of the white area which on October 4 was just to the right of and below it; but this, of course, would not explain the spot's actual lengthening. By October 22 the spot had become indistinguishable by ultra-violet and violet light, but was sensibly bright by extreme red light. On December 12 it was very bright in the last-named colour, apparently the brightest part of the planet. The northern part of it was bright by violet light, and the extreme northern edge by ultra-violet. It must at that time have presented a lurid appearance in the telescope, an outstanding crimson marking shading to yellow or white on its northern edge. On the extreme red photograph for this date there is a curious dark spot nearby with ramifying tentacles, which is represented by hazier forms in the violet and ultra-violet photographs. Other chromatic unconformities are revealed by the illustrations.

Circumstances of the Disturbance in the South Equatorial Belt.—A number of the travelling dark spots referred to by Mr. Phillips and his associates are shown in figs. 13 to 16, Plate 11, and in some of the illustrations of Plate 10. As has already been stated, these moved rapidly in a retrograde direction. There is little to add to the account given of them by the above authors, except perhaps the comment that they are dark in photographs of all colours (compare figs. 2 and 6, also 13, 14, and 15), a fact that of course contributed to their visibility in the telescope. Their presence in the ultra-violet photographs may be taken to indicate that they existed in the upper atmosphere at least,

* For a description of methods employed, etc., see *L.O.B.*, **8**, 50, 1927.

† *L.O.B.*, **8**, 50, 1927; *P.A.S.P.*, **39**, 358, 1927; *M.N.*, **88**, 709, 1928.

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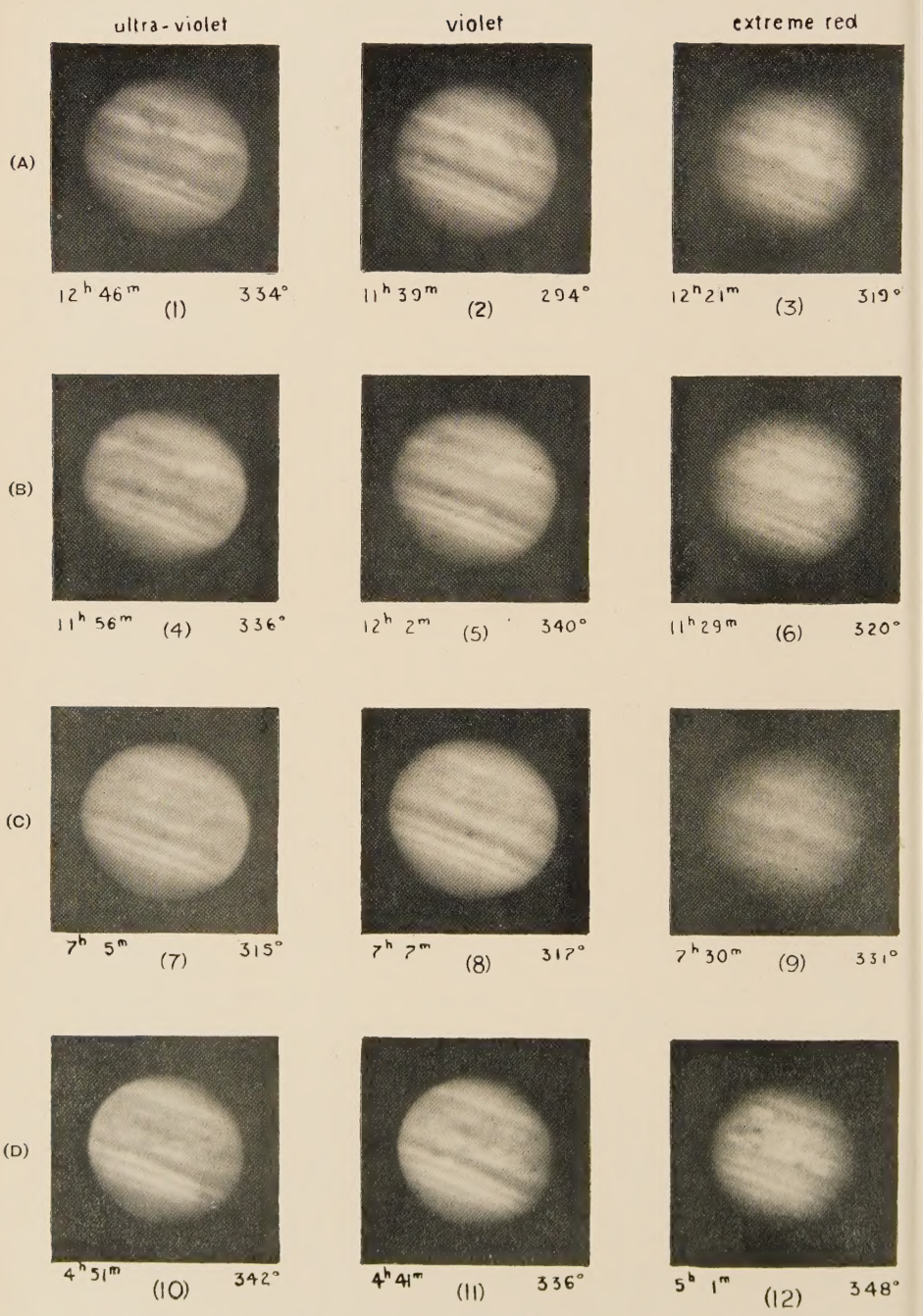
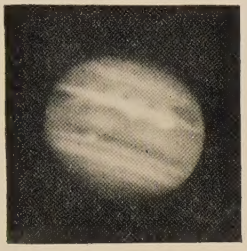


Photographs of Jupiter by Light of Different Colours, 1928.

(A) G.C.T. 1928 Oct. 4. (B) Oct. 9. (C) Oct. 22. (D) Dec. 12.

ultra-violet

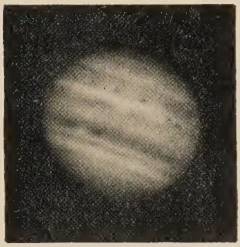
G.C.T. Oct. 6



$12^h\,43^m$ (13) 273°

violet

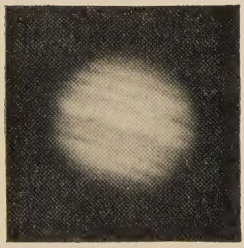
G.C.T. Oct. 6



$12^h\,50^m$ (14) 277°

ext. red

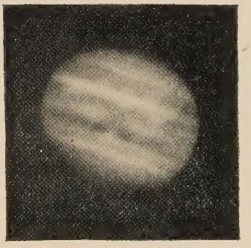
Oct. 6



$12^h\,59^m$ (15) 283°

u-violet

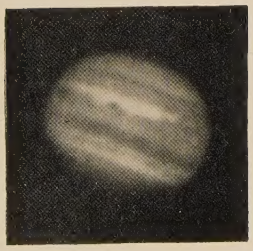
Oct. 9



$9^h\,42^m$ (16) 255°

u-violet

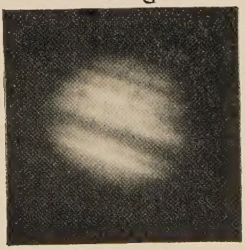
Oct. 28



$8^h\,11^m$ (17) 178°

orange

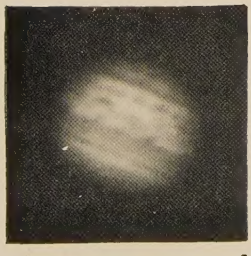
Oct. 28



$8^h\,37^m$ (18) 193°

ext-red

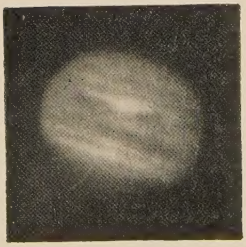
Oct. 28



$8^h\,18^m$ (19) 182°

u-violet

Nov. 9



$6^h\,24^m$ (20) 118°

Photographs of Jupiter by Light of Different Colours, 1928.

while their occurrence in colours of longer wave-length may be due to their downward extension into the lower levels, of which these colours are assumed to render a general picture, or, on the other hand, to opacity for all colours of whatever produced them in the upper atmosphere. Other dark spots and areas have quite different colour reactions, as noted elsewhere in this paper.

Of more immediate interest than the dark spots, because the phenomenon was not so completely covered by the telescopic observations, is the movement of markings in the *preceding* direction. This was studied, by the observers referred to, principally through the motion of "a rather small white spot at the *following* end of a rapidly moving break or gap in the northern component of the belt." The photographs show that this forward movement was partaken of by all of the great white area contiguous to the red spot, which will for convenience be called a cloud, that forms so conspicuous a feature of the four photographs in the upper left corner of Plate 10. The displacement (toward the left with respect to the red spot) of the cloud is shown in these photographs and can be followed in figs. 13, 14, and 16 of Plate 11, where a convenient point of reference in longitude is had in the white and black marking in the north equatorial belt, which moves approximately with System II. The motion of the white area, or cloud, from measurements of the middle protuberance on its northern edge in the photographs of October 4, 6, and 9, was approximately 7° per day with respect to System II. This is approximately the velocity found by Messrs. Hargreaves, Peek, and Phillips. The motion of the cloud as a whole seems to have been somewhat less than this, as will be seen later. The following considerations make it appear probable that it was this cloud that later manifested itself as the "irradiating spot" on the planet's limb.

The authors' description of the latter object is of "a white spot in latitude slightly south of the northern component of the south equatorial belt, which appeared as a bright projection or irradiating spot at the limbs but was much less conspicuous, or unidentifiable, when well on the disc. . . . The observations obtained indicate that it was visible when more than 70° from the central meridian, and that it was rotating approximately at the rate of System I. The object was similar to those observed in 1919 . . . but slightly further south." The most likely explanation that presents itself of the phenomenon thus described is that the object observed was especially bright in light of the shorter wave-lengths, since photographs taken by such light suffer comparatively little weakening at the limb. A blue or violet marking would therefore be relatively brighter, and consequently more easily seen, when near the limb than when on the disc, for much the same reason that transiting satellites are best seen there. An examination of violet and ultra-violet photographs substantiates this explanation. Circumstantial accounts of the observations of the spot are given in the paper under consideration only for October 24 and 25, but these indicate its longitude to be 212°± (System II.), as of date October 25. Our closest plates covering this neighbourhood were taken on October 28·3. Inasmuch as the spot was

reported as moving with System I., which loses 7°·5 of longitude every day with respect to System II., its longitude by System II. at this time should have been 212° − 23° = 189°. This is not far from the brightest part of an elongated white cloud photographed on October 28 and shown in fig. 17, Plate 11. The feature appears much less strikingly in the orange photograph and is visible only with difficulty in the one taken by extreme red light. (Compare figs. 17, 18, and 19. A convenient indicator of the position of the marking in the orange and red photographs is the dark spot that makes the large indentation in the ultra-violet image of the cloud.) The next photographs covering the appropriate longitude are those of November 9·3. The violet and ultra-violet photographs (fig. 20) show what may be assumed to be the same object, greatly reduced in extent, lying in approximate longitude 117°. The alteration in form between the two dates is so great that it is impossible to identify homologous parts of the marking for the purpose of measuring its velocity, but settings on the most conspicuous part for each date (just to the right of the indentation in fig. 17 and on the centre in fig. 20) yield a value of − 5°·4 per day with respect to System II. Assuming this velocity and calculating back to the preceding observation in the requisite longitude, we find a longitude of 286° for October 9 (fig. 16). This is the position of the strong lobe on the right extremity of the bright cloud. The correspondence is close enough to make it very probable that the elongated "clouds" shown in figs. 16, 17, 20, and some of the other illustrations are identical. The extrapolation is, however, through 19 days in time and more than 100° in longitude, and it would be well to have the advantage of confirmation of the continuity of motion by intermediate photographs if such have been secured elsewhere. Assuming the identity, the observations indicate that the cloud as a whole swept forward relatively to System II. at the rate of about 5° per day, while the northern protuberances, in latitude − 6°, had a velocity of approximately 7°, the higher value no doubt being due to their proximity to the equator. If these inferences are correct, the irradiating white spot of October was identical with the great white region that preceded the red spot in the earlier days of the month. It is surprising, therefore, that it was not observed sooner on one of its passages over the limb. That it was not may have been the consequence of oversight, or is perhaps to be explained by the fact that Jupiter came to opposition late in October, at about the time the irradiating spot was first detected. Opposition undoubtedly provides the most favourable condition for seeing an object of this kind, because the limb is then fainter by light of the longer wave-lengths than at any other time, and a bright object projected upon it would be the more conspicuous.

From the length of time during which the spot remained visible in an apparition—some 25 minutes—Mr. Phillips and his associates concluded that it was distinguishable until within 70° of the central meridian. However, the length of the marking was so great that it must have taken quite that length of time for the brighter part to cross the limb, so that it is possible that it was visible only at, or very near, the limb, its apparent

persistence being due to the coming into view of successive portions. The authors refer to observations of irradiating spots at earlier oppositions, and it is interesting to note the great range in the values of longitude derived for such an object.* This was remarked by Mr. Phillips, one of the observers. The length of the actual marking in the present instance supplies a plausible explanation of the uncertainty of such determinations : different parts of it might well be the subject of observation at different apparitions.

Under the interpretation of the ultra-violet photographs as pictorial representations of the planet's outer atmospheric layer, the marking under discussion is judged to have been a high-level phenomenon, a conclusion to which Mr. Phillips had already come through a consideration of the simple but convincing analogy with the visibility of faculæ near the sun's limb.† We are thus afforded some corroboration of the idea that short-wave photographs provide records of a planet's upper atmospheric levels, and more especially of an inference, drawn in another paper, with respect to the south polar cap of Mars. The cap becomes relatively richer in light of short wave-lengths as it approaches the limb, a circumstance that was ascribed to the presence of a haze cap high up in the Martian atmosphere. It will be interesting to note, in such future observations of bright spots on the limb of Jupiter as may be made, whether the colour consistently favours the more refrangible part of the spectrum. There would seem to be nothing impossible in the occurrence of a yellow or red spot at a considerable altitude, and in such a case we should not expect it to fade so rapidly on approaching the limb as do other parts of the image in those deep colours. When close to the limb such an object should appear as an excrescence on the edge of the image photographed in its own colour. The fact that this appearance has not been observed in photographs taken by light of the less refrangible colours renders it probable that these colours are found only at depth.

Comments on Other Features of the Photographs.—There are a number of spots, both bright and dark, in the north equatorial belt that deserve mention, especially since two of them might easily be mistaken for a satellite and its shadow in transit over the disc. This pair is visible in all of the photographs of the first two columns of Plate 10, while only the bright one is discernible in those of the third column, taken with light of greater wave-length. The dark member advanced slowly with respect to System II., and the white one somewhat faster, the result being that they gradually approached one another as shown in the lower of the illustrations referred to. The positions and motions, with respect to System II., are as follows :—

	Long., Nov. 1·0.	Motion in Long. per Day.
	°	°
Dark spot	317	−0·23
White spot	339	−0·46

* *B.A.A. Mem.*, **26**, pt. 3, 59–60, 1925.
† *Ibid.*

The bright component appears to be about equally strong in all colours employed. The dark one is more pronounced in the mid-range of colour, violet, green, and orange; it is fainter in ultra-violet and red, and scarcely visible in extreme red. Another persistent marking, in substantially the same latitude, was a white spot which was flanked on either side by short, narrow, dark lines in slightly lower latitude. It is best shown in fig. 14. The *following* dark line was a little farther from the spot than the *preceding*. The situation of the dark and bright elements of this configuration, with respect to the north equatorial belt, is almost identically that of the pair of spots just described, and one might have expected to find the same relative motion, but in this instance the dark and bright components moved together, the configuration maintaining itself unaltered from October 4 to December 21, which were the first and last dates of observation of this part of the planet. The longitude of the central white spot and the motion of the system were :

Longitude, Nov. 1·0, 246° ; motion − 0°·24 per day ; System II.

The colours of the bright and dark members are the same as those of the pair previously referred to. The bright spot is readily visible in all colours. The flanking dark spots, or lines, are most easily seen in the range of colour from violet to orange ; they are less conspicuous by ultra-violet and red light, and are faint, or invisible, by light of the extreme red.

NOTE ON PLATES 10 AND 11.*

Figs. 1 and 9 have been retouched for reproduction. The negative used for fig. 1 has two blemishes, one near the left extremity of the equatorial zone, the other in the south temperate and polar regions to the right of the red spot, both due to some foreign substance that lodged temporarily on the filter. In fig. 9 a small, disfiguring scar was removed from the red spot.

Further Notes on the Recent Disturbance on Jupiter.

By F. J. Hargreaves. (Plate 12.)

A preliminary account of the earlier phases of the disturbance in the southern hemisphere of Jupiter was given in a joint paper by the Rev. T. E. R. Phillips, Mr. B. M. Peek, and the present writer, appearing in *M.N.*, 89, 209. This account was necessarily incomplete owing to the fact that at the time of writing rapid changes were still in progress and masses of observations recently made had not been reduced and studied. In particular, only a very brief reference was made to an unusual phenomenon observed in the last few days of October, namely, an extremely bright white spot apparently projecting at the limbs of the planet in the latitude of the south equatorial belt.

At the time there was considerable doubt whether the cause of this appearance was visible at all on the central meridian, and so it was referred to as " much less conspicuous, or unidentifiable, when well

* In comparing the Plates with the text, it should be borne in mind that perfect reproduction of the photographs has not been possible.—*Editors.*

on the disc " ; but further consideration of the recorded observations has shown that this statement is misleading, and the concurrent appearance of Mr. W. H. Wright's most interesting paper (*M.N.*, **89**, 703), in which considerable attention is given to the phenomenon, presents a favourable opportunity of supplementing the former brief account.

The spot was first observed at the *preceding* limb on October 24 from $15^h\ 40^m$ to $16^h\ 0^m$ (the day is reckoned to begin at Greenwich Mean Noon, as in the former paper). The next evening it was seen at the *following* limb from $7^h\ 28^m$ to $7^h\ 50^m$, and again at the *preceding* limb from $11^h\ 18^m$ to $11^h\ 45^m$. These two latter observations suggested that its longitude was 218°·6 (System I.), corresponding to 212°·5 (System II.). On the same night (October 25) a long, double-lobed, very bright area was observed between the components of the south equatorial belt, the *following* end of which area consisted of a still brighter round spot, which was judged to be on the central meridian at $9^h\ 27^m$ (longitude 213°·7, System I.). At the time it did not seem probable that any part of this bright area, not even the extremely bright spot at its *following* end, could be responsible for the appearance at the limb ; the discrepancy of 5° of longitude seemed too great. Unfortunately this was the only occasion when the spot could be observed consecutively at all three phases—namely, at the *f* limb, the central meridian, and the *p* limb—and consequently the evidence regarding identity is scanty.

All the available observations are tabulated below, the wording in inverted commas being the description recorded at the time, or so much of it as is relevant. The times A, A′, B, C, C′ are respectively the first appearance at the *f* limb, the last appearance thereat, the time of central meridian passage of the bright spot mentioned above, the first appearance at the *p* limb, and the last appearance thereat.

The pictorial chart (Plate 12) is on the same plan as Plate 4 of the former paper, with the difference that the longitudes are those of System I., the rotation period of which is $9^h\ 50^m{\cdot}50$, instead of System II. (The present is a suitable opportunity for correcting an error in Plate 4 of the former paper, in which the sketch for October 24 was incorrectly designated October 25 and incorrectly placed in the position appropriate for the latter date.)

The uniform retrogression of the conspicuous white spot is shown by the broken line, and the arrow above the first sketch shows the longitude, as deduced at the time from the limb observations, of the cause of the irradiation at the limbs. As already mentioned, the discrepancy of about 5° made it very doubtful whether the white spot at longitude 213°·7 was the cause of the irradiation, but it now seems likely that this identification is correct and that too much weight was given at first to the longitude derived from the two consecutive limb observations of October 25.

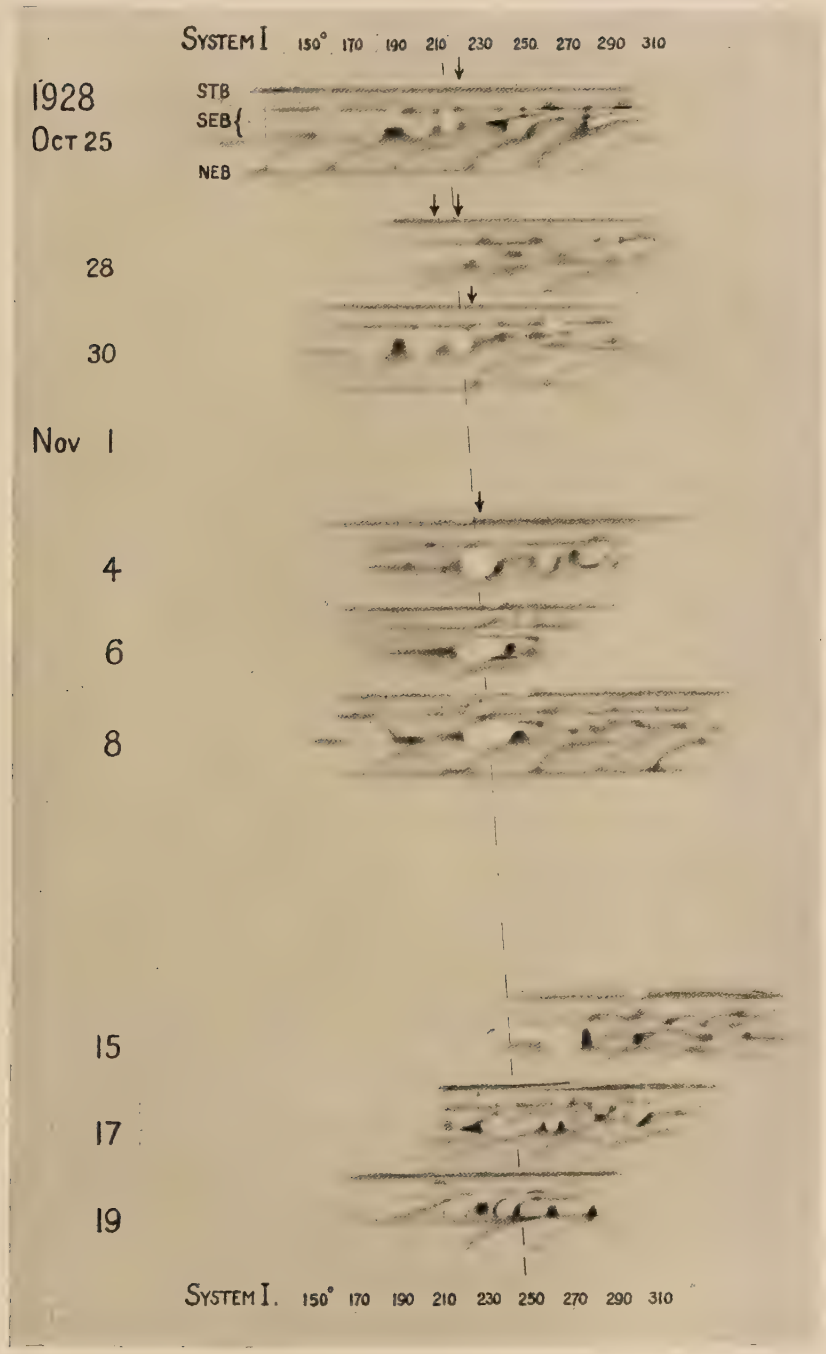
It is unlikely that the longitudes from central-meridian observations are much in error—they lie so nearly on a straight line as to suggest that they are fairly consistent among themselves, and, in so far as the single observer responsible for them is concerned, he was usually two

or three minutes later than his collaborators in his estimates of central-meridian passage, so that if any personal error is present, its elimination would be likely to increase the discrepancy. All the observations were made within a few days of the date of opposition, so that phase does not come into question.

Date. 1928.	A. h m	A′. h m	B. h m	C. h m	C′. h m
Oct. 24	..	..	..	15 40	16 0±
25	7 28 (Irradiating.)	7 50	9 27 "Very bright white spot."	11 18 (Irradiating.)	11 45
28	..	..	..	8 10 (Irradiating.)	8 31
	13 53 (Irradiating.)	14 20			
30	..	..	7 42 "V.v. bright white spot."	9 25 (Irradiating.)	10 2
Nov. 4	.. "No sign of irradiating spot."	..	5 57 "Extremely bright white spot. Far brighter than any other region on the disc, and pure white in colour. By contrast, rest of disc looks dull, tawny colour."	7 43 "No projection."	8 5±
5	9 33 "Not conspicuous — no projection."	9 55	.. (Seen, but not on C.M.)	..	..
6	..	..	7 12 "As bright as on Nov. 4 (? brighter) and much larger."	..	..
8	..	..	8 29 "Large v. bright white region. Not so bright as on 6th. P. half is the brighter."	..	10 36 "10–36. The seeing has been too bad to see bright spot on terminator, but have now just caught last glimmer. Probably not nearly so bright as a few days ago, or it would have been conspicuous even in bad seeing."

In dealing with the four "solitary" or "unpaired" observations of October 24, October 28 (two), and November 5, either one may assume that the cause of the irradiation was not necessarily connected with the bright spot and try to determine its longitude from the limb observations alone, or one may assume that the bright spot was the cause and accept as real the inequality between the intervals observed on October 25. All the observed intervals and their means are as follows :—

	B–A. h m	B–A′. h m	C–B. h m	C′–B. h m
Oct 25	1 59	1 37	1 51	2 18
30	..	..	1 43	2 20
Nov. 4	..	..	1 46	2 8
8	..	..	..	2 7
Means	1 59	1 37	1 46·6	2 13·3


SYSTEM I 150° 170 190 210 230 250 270 290 310
1928
OCT 25
STB
SEB
NEB
28
30
NOV 1
4
6
8
15
17
19
SYSTEM I. 150° 170 190 210 230 250 270 290 310

On the first assumption the time-interval to be applied to the mid-instants of all the "unpaired" observations is $1^h\ 54^m$, whether they were made at the *p* limb or the *f* limb. The arrows just above the sketches for October 28 and 30 and November 4 indicate the longitudes thus obtained. Ignoring for the present the left-hand arrow for October 28, of the remaining four two are outside the confines of the bright spot altogether and two are within.

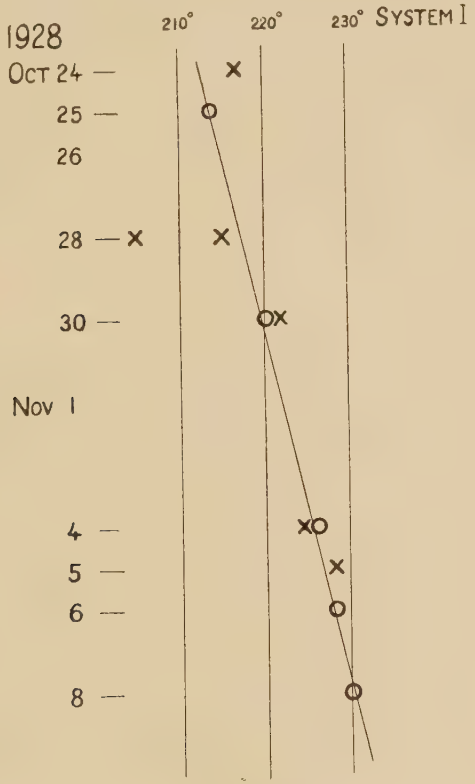


Fig. 1.

The results obtained on the second assumption are shown separately (to avoid confusion on the plate) in fig. 1, where the crosses indicate longitudes obtained by applying the mean unequal intervals $1^h\ 48^m$ and $2^h\ 0^m$, and the circles indicate the longitudes of the centre of the bright spot (B) obtained from central-meridian observations. With the one exception, all the crosses lie well within the confines of the bright spot.

The discrepant longitude for October 28 corresponds to the observation $13^h\ 53^m$ to $14^h\ 20^m$. On this night a note was made at $7^h\ 30^m$ (well after central-meridian passage) that "the white area S.E.B. is

extremely bright. It extends a very considerable distance in longitude." It was also noted that the projection at the limb was on both occasions distinctly further north than on previous occasions. It is therefore probable that a different part of the general " white cloud " (see figs. 17, 18, 19, Plate 11 of Mr. Wright's paper) was responsible for at least a part of the irradiation observed on that date.

Although the evidence is not conclusive, it is in favour of the identity of the white spot (B) with the cause of the irradiation (except for the occasion on October 28 noted above), and it suggests that this spot had for a few days the property of becoming specially visible at a variable angle (in the neighbourhood of 70°) from the central meridian for a variable time, this angle being greater on the *preceding* side of the disc than on the *following* side. Further, the property of irradiating at the limb seems to have been lost before the spot had begun to fade—possibly before it reached its maximum brilliancy—as seen on the central meridian. The observations are not inconsistent with the supposition that a temporary gap was present in an outer absorbing layer in the planet's atmosphere, of which the *preceding* edge was at a greater altitude than the *following* edge.

It will be evident that the lengths of the intervals do not support the idea that the irradiating object was ever seen at the true limb, nor do the observations support Mr. Wright's suggestion that the whole of the general white cloud of October 24 to 28 was the cause.

The *preceding* part of the cloud faded away as the spot forming its *following* end became larger and more brilliant, and by November 17 the spot itself had begun to be invaded by dark matter (see Plate 12), which on November 19 became very prominent, although the two dark spots flanking it persisted.

The sketch for November 15 is perhaps not strictly relevant, but it shows a transient white area (noted as " very bright ") centred in longitude 265° which extended right into the equatorial zone. Two days later it had vanished. The whole sketch, when compared with that of November 17, provides an example of the astonishingly rapid changes which were still occurring at that time.

It may perhaps be worth mentioning that a careful watch was kept on objects near the limb from the beginning of the disturbance, as the general turbulence was so strong as to suggest that unusual limb phenomena might occur. For some time previous to October 24 a considerable extent in longitude of the space between the two components of the S.E.B. had been noted as rather bright, but it is highly probable that if any phenomenon as striking as that which obtruded itself on the attention on October 25 had occurred earlier during observing hours it would not have been missed.

Mention was made in the former paper of the visibility of the northern component of the S.E.B. when near the limb. Another striking instance of a like appearance was repeatedly noted in the case of the two conspicuous dark streaks, straddling a white spot, extending along the northern component of the N.E.B. These are referred to in the latter part of Mr. Wright's paper. They were a deep

rich red in colour and seemed to be scarcely affected at all by limb-haze. On two occasions (December 8 and 23) the *preceding* spot was seen close to the limb so foreshortened as to appear as an exceedingly fine and very short dark line *parallel* to the limb. On December 8 it was thus seen when 72° from the apparent central meridian and on December 23 when 79°·2 therefrom, both these observations being made with the 18-inch reflector. On November 20 the other spot was seen with the 6½-inch reflector when 67° from the apparent central meridian. A single dark red spot in the same latitude, also referred to in Mr. Wright's paper, had the same property. These objects therefore appear to be examples of *red* spots at a considerable altitude. They would not be likely to be observed near the limb photographically owing to their feeble luminosity.

Some strikingly blue or blue-green objects of rather low luminosity were seen along the southern edge of the N.E.B. and in the equatorial zone (notably on December 8 between longitudes 300° and 10°, System I.), but they faded away soon after central-meridian passage in quite a normal manner, suggesting that they were at a considerable depth.

Reference is made in Mr. Wright's paper to the change in the colour of the Red Spot, culminating about the middle of December. At that time the spot appeared very bright and light pink in colour, particularly towards the south. Mr. Wright does not say whether this object was as inconspicuous in the green photographs as in the violet, but, if so, the inference to be drawn from the photographs would tally with the visual observations. Certainly the spot was never seen crimson at any time during the apparition.

The three visual observers feel very gratified at the ready and ample compliance with their request for photographic observations, and by the confirmation and amplification of their own observations which the photographs supply.

L'Enregistrement des Oscillations d'un Pendule sans Liaisons Matérielles. By Gustave Ferrié, Assoc.R.A.S.

La possibilité d'enregistrer sur un chronographe les oscillations d'un pendule sans avoir recours à aucun lien matériel susceptible de troubler son mouvement constituerait un progrès important dans l'étude de certaines questions d'astronomie et de géophysique.

Mais pendant de longues années ce problème n'a reçu aucune solution. Nous rappellerons sommairement le procédé employé encore presque universellement jusqu'à ce jour pour enregistrer à distance les battements des pendules astronomiques. Le balancier porte un cliquet qui, toutes les deux secondes, au moment du passage par la verticale entraîne un rochet. Le mouvement de ce dernier organe ferme un contact électrique et la courant produit agit sur la plume du

chronographe. Ce système excellent théoriquement présente divers inconvénients pratiques.

Malgré la perfection atteinte aujourd'hui par l'art de l'horlogerie, il est absolument impossible de donner à toutes les dents du rochet, une forme identique. Il en résulte que l'action du cliquet ne se produit pas toujours au même instant de la période du balancier ce qui a pour conséquence tout d'abord d'apporter un trouble dans le fonctionnement de l'appareil, puisque la perturbation provoquée par le fonctionnement du mécanisme ne se produit pas exactement au passage par la verticale.

Mais l'inconvénient le plus grave c'est que l'origine des différentes secondes enregistrées ne correspondant pas à des instants identiques de la période du balancier, celles-ci sont inégales comme on s'en aperçoit facilement en enregistrant par exemple le mouvement d'un diapason sur le même chronographe et en évaluant en fonction de la période de ce diapason la durée de diverses secondes consécutives.

On constate * avec les meilleures pendules astronomiques des différences entre des secondes consécutives atteignant parfois le centième de seconde. Une certaine périodicité dans ces irrégularités montre bien qu'elles sont dues à des différences entre les diverses dents du rochet. Ces écarts entre les diverses secondes des horloges ne présentent pas évidemment une grande importance au point de vue de l'astronomie pure, ils sont encore bien inférieurs aux erreurs commises dans l'évaluation de l'instant du passage des astres au méridien, mais ils deviennent fort gênants lorsqu'on a besoin de connaître avec une haute précision un temps assez court.

Quoiqu'il en soit, le procédé que nous venons de rappeler constitue une solution suffisante pour bien des cas du problème de l'enregistrement à distance des oscillations du balancier d'une pendule astronomique.

Mais le problème se pose dans tout son entier pour les pendules sur lesquels il est impossible de faire agir le moindre lien matériel sans troubler leur fonctionnement : nous voulons parler des pendules libres servant à la détermination de la constante de la gravitation. On n'avait trouvé rien de mieux pour leur observation que la méthode des coïncidences, imaginée il y a bien longtemps par Borda et qui présentait l'inconvénient d'être d'un emploi fort long. Il en résultait par conséquent qu'il n'était possible par ce procédé que de déterminer une valeur moyenne de la constante de la gravitation et que si celle-ci, comme cela est probable, est soumise à des variations périodiques (marées de l'écorce terrestre, etc.) il était impossible de les mettre en évidence.

Depuis quelques années pourtant les propriétés des lampes à trois électrodes ont apporté diverses solutions au problème de l'enregistrement à distance sans lien matériel des oscillations d'un pendule, solutions qui commencent à entrer dans la vue de la réalisation pratique.

Nous voudrions tout d'abord rappeler que dès 1922 nous avions cherché, avec le collaboration de M. Jouaust, à enregistrer le mouvement du pendule de gravitation par l'artifice suivant : le pendule était porté à un potentiel négatif assez élevé et, lors de son passage par la

* G. Ferrié et R. Jouaust, *Comptes-Rendus*, t. clxxx. p. 1145, 1925.

verticale, la pointe inférieure terminale venait agir par influence sur une pointe fixe placée dans sa cage et reliée à la grille du premier triode d'un système amplificateur. Il en résultait une variation brusque du courant plaque de ce triode, variation qu'amplifiaient les lampes suivantes et qu'il était possible d'enregistrer simultanément avec les battements d'une pendule ou une émission de signaux horaires.

Des expériences entreprises en collaboration avec le Service Géographique de l'Armée française sur un pendule Defforges montrèrent que ce dispositif n'altérait en rien la période des oscillations du pendule libre. Les résultats de ces premières expériences furent communiqués à l'Assemblée Générale de la Section de Géodésie de l'Union de Géophysique à Rome.* Plus récemment nous avons été le premier à préconiser l'emploi combiné des cellules photo-électriques et des lampes à trois électrodes.† Nous avons appliqué ce procédé à une des pendules astronomiques placées dans les caves de l'Observatoire de Paris. Il a été également appliqué sous notre direction par les Officiers du Service Géographique de l'Armée au pendule de gravitation et depuis a été employé à plusieurs reprises à l'étranger par divers expérimentateurs.

Dans ce qui va suivre, nous indiquerons un procédé ne nécessitant que l'emploi de triodes.

Quoiqu'il ne nous semble pas présenter tous les avantages qu'offre l'emploi des cellules photoélectriques, il peut rendre des services dans certains cas particuliers et c'est ce qui nous a conduit à le décrire.‡

Le principe de cette méthode est le suivant : dans tout dispositif utilisant des lampes triodes pour la production d'oscillations de haute fréquence, le courant plaque des lampes a des valeurs notablement différentes suivant que le système est ou n'est pas en état d'oscillation.

D'autre part il suffit très souvent d'approcher une pièce métallique d'une des bobines d'inductance de l'oscillateur à lampe pour provoquer non pas un arrêt complet des oscillations, mais une diminution telle de leur amplitude qu'il en résulte une variation importante du courant plaque des lampes. La raison en est la suivante : les courants de haute fréquence provoquent dans la pièce métallique des courants induits et la consommation d'énergie dégradée à l'état d'effet Joule par ces courants a pour conséquence une augmentation apparente de la résistance du circuit oscillant du dispositif à lampes. On voit donc qu'il suffit de fixer au pendule dont on veut enregistrer les oscillations une plaque métallique qui une fois par période (de préférence au moment du passage par la verticale) se trouve au voisinage de la bobine d'inductance de l'oscillateur. Il se produit à ce moment une variation du courant plaque des lampes qui est susceptible d'actionner la plume d'un enregistreur à action magnétique dont les enroulements se trouvent intercalés dans le circuit filament plaque.

* G. Ferrié et G. Perrier, *Comptes-Rendus de la 1° Assemblée Générale de la Section de Géodésie de l' Union Internationale de Géodésie et de Géophysique,* Annexe 14, p. 168.

† G. Ferrié, *Onde Electrique,* Mars 1925, p. 97.

‡ " D'autres procédés n'utilisant que des triodes ont été indiqués par Lejay, *Journal des Observateurs,* vol. xi. p. 201, Décembre 1928.

Ce procédé théoriquement très simple n'est pas sans présenter certaines difficultés de réalisation pratique.

Tout d'abord ces phénomènes d'amorçage et de désamorçage des oscillateurs à lampes ne sont pas instantanés.

Comme tous les phénomènes électriques, ils se produisent suivant une loi exponentielle. Il était nécessaire pour la précision des mesures de rendre cette période très brève, en tous les cas plus courte que le temps minimum qu'il est possible d'évaluer sur les enregistrements c'est à dire dans le cas actuel inférieur au millième de seconde.

C'est ce qui nous a conduit à utiliser des oscillations de très haute fréquence : 50 millions de période par seconde ($\lambda = 6$ mètres). D'autre part, la pièce métallique devait avoir une forme telle que la durée de son action sur le pendule fut aussi courte que possible.

Cette pièce métallique devait en outre pouvoir être fixée à un pendule sans modifier notablement les conditions de son fonctionnement.

Pratiquement nous avons cherché à utiliser l'appendice en forme de couteau qui termine les pendules géodésiques de Defforges et qui sert précisément à la détermination des coïncidences.

Ces considérations nous ont amené à modifier nos directives primitives en nous basant sur les remarques suivantes :

1°. On provoque une diminution très importante de l'amplitude des oscillations d'un émetteur à lampes en approchant du circuit oscillant un deuxième circuit accordé sur la fréquence propre des oscillations.

2°. Avec les ondes courtes employées on réalise facilement un circuit accordé.

Un semblable circuit accordé est représenté en AB sur la figure.

Un conducteur de cuivre est replié suivant la forme indiquée et aux deux extrémités sont fixés deux petits conducteurs isolés. On accorde le circuit sur la fréquence utilisée en torsadant plus ou moins les deux conducteurs isolés.

L'inductance de ce circuit est constituée en majeure partie par les spires S_1 et la capacité par la capacité relative des deux fils torsadés et aussi par la capacité relative des deux conducteurs *ae* et *cf*.

Lorsqu'une pièce métallique vient à passer entre ces deux conducteurs, elle modifie donc la période propre du circuit oscillant qui cesse d'être accordé sur la fréquence de l'oscillateur. Le montage finalement réalisé est indiqué dans la figure.

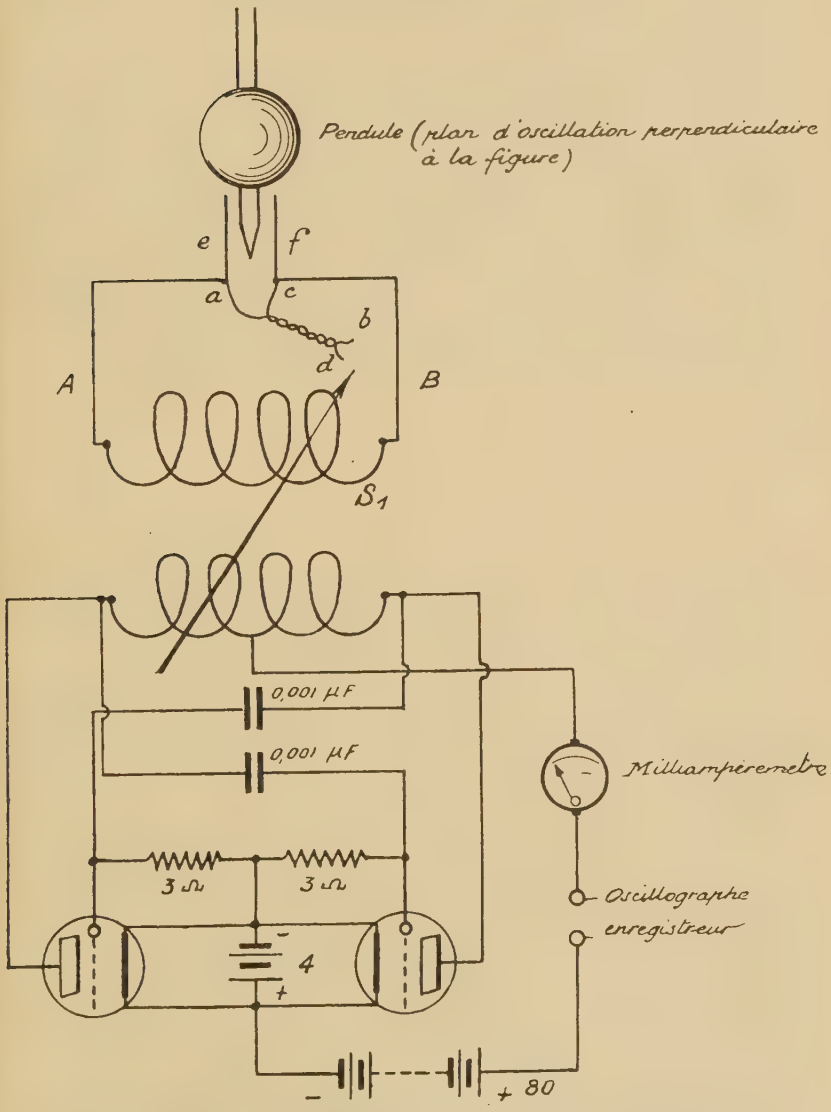
L'oscillateur à lampes est du montage symétrique connu en France sous le nom de montage Mesny, les capacités des lampes et des divers conducteurs constituent la capacité du circuit oscillant.

Cet oscillateur est de dimensions très réduites. Il peut sans difficulté être placé dans la cage d'un pendule géodésique genre Defforges.

Il suffit simplement que cette cage présente des ouvertures permettant l'introduction des conducteurs d'alimentation des batteries. Avec le montage symétrique ces conducteurs ne jouent aucun rôle dans la production des oscillations et leur longueur n'intervient pas.

On peut du reste remarquer qu'avec le procédé d'enregistrement que nous préconisons, les déterminations de la constante de la gravitation peuvent se faire en un temps très court et il n'est pas nécessaire de faire

Enregistrement d'Oscillations Pendulaires.



Circuit oscillant : 4 spires rectangulaires 1 cm. × 6 cm. avec prise médiane. La capacité oscillante est la capacité résiduelle des lampes et des inductances. $\lambda = 5$ à 6 mètres.

osciller le pendule dans le vide. Le fonctionnement du système est finalement le suivant :

L'oscillateur à lampes est couplé au circuit accordé :

Dans ces conditions, l'amplitude des oscillations n'a qu'une valeur très minime.

Le circuit accordé est placé au-dessous du pendule de telle façon qu'au passage par la verticale l'appendice en forme de couteau dont nous avons parlé plus haut passe entre les deux fils *ae* et *fc*.

Ce passage est évidemment de très courte durée, mais pendant ce temps très court, le circuit d'absorption n'est plus en résonance, l'amplitude des oscillations du dispositif à lampes atteint une grande valeur et le courant continu du circuit anodique des lampes subit de ce fait une variation de plusieurs milliampères, suffisante pour actionner la plume d'un enregistreur à noir de fumée, comme ceux utilisés par exemple à l'Observatoire de Paris pour les enregistrements de signaux horaires.

Des expériences faites avec le dispositif que nous venons de décrire ont montré qu'il était possible de noter le passage du pendule par la verticale avec une précision du millième de seconde.

1929 *June.*

The Mechanics of the Chromosphere. By W. H. McCrea.

The problem of the mechanical state of the solar chromosphere is well known. For a long time it was impossible to explain why such an atmosphere does not collapse under gravitational forces to a mere fraction of its actual thickness. Then Milne put forward his theory of selective radiation-pressure in the case of ionised calcium, which spectroscopically is the most prominent constituent of the chromosphere. This theory has had many successes. The difficulty is that it seems too special to extend to gases like hydrogen and helium which are also present in relatively great abundance.

The failure to find a general solution seems to be due to our hitherto having simplified the problem to too great an extent from the hydrodynamical point of view. The necessity for the use of more general hydrodynamics in all astrophysical applications has recently been emphasised by Rosseland. The present paper is a first attempt to take this into account in the problem of the chromosphere. The factor which chiefly concerns us is the turbulence and its effect on the laws of gas-pressure. By allowing for this it seems possible to derive very simply a satisfactory general solution.

The tentative character of much of the investigation should not, however, be overlooked.

1. *The Problem of the Equilibrium of the Hydrogen Chromosphere.*—I have recently * given an outline of this problem. It appeared that ordinary gravitational equilibrium offers no solution. For the un-ionised

* McCrea, *M.N.*, **89**, 483 (1929). See especially § 8, p. 494.

part of the gas it always gives a density law $\exp\left(-\frac{Hgy}{kT}\right)$, where the symbols have the same meaning as in the paper quoted. This is about $\exp\left(-4600\frac{y}{a}\right)$ for T = 5000°, the probable temperature. Every possibility involving radiation-pressure which could be thought of was tried, but without success. I finally concluded that electrical forces remained as the sole possibility. These have since been examined, but they too offer no solution. It is hoped on a later occasion to outline the reasons for this since it concerns the problem of the corona also.

It consequently seems impossible to consider the hydrogen as in *statical* equilibrium. This would appear to be true for many other gases in the chromosphere. As soon as we remove this restriction and allow for the effects of the observed turbulence of the gas, a very simple explanation seems to offer itself.

2. *The Importance of Turbulence in the Solar Atmosphere.*—Two authors working along entirely different lines have recently independently arrived at this importance.

Rosseland * considers the general principles governing the transition from laminar to turbulent motion, and shows that if there are any relative mass motions at all in the sun and stars they must be turbulent motions. He points out that we actually *see* it in the sun's atmosphere in the phenomena of the "granulations." But according to what has just been said, were we to find evidence of any relative motion we should have to conclude there was turbulence present; and, of course, we do see such motions in the behaviour of spots and faculæ, in the shifts of spectral lines, and in the prominences. The last named make it particularly clear, for Rosseland suggests that we see going on in the changes in eruptive prominences the actual *transformation* of laminar into turbulent motion.

The viscosity, diffusion, and conduction of the earth's atmosphere and oceans are influenced to an astonishingly large extent by the comparatively feeble turbulence present. This has actually been measured. Rosseland appeals to this to show the great importance of turbulence, especially as regards viscosity, in the stars in general.

We quote in the second place the work of Unsöld. He has measured † the contours of the H, K lines of Ca^+ in emission in the solar chromosphere and the corresponding parts, H_3, K_3, of the absorption lines. He finds the contours are only to be explained by a Doppler effect given by a mean velocity of 15 km./sec. This is about ten times the mean temperature velocity of the atoms concerned, and he ascribes it to turbulence. The results he derives provide good evidence of the truth of his hypothesis.

As a matter of fact he attributes the turbulence to the fluctuations of the radiation-pressure acting on the calcium. But Rosseland's work suggests some more general cause. In the first place, the fluctuation in the radiation to which Unsöld appeals is likely to be itself caused

* Rosseland, *M.N.*, **89**, 49 (1929). † Unsöld, *Ap. J.*, **69**, 207 (1929).

by turbulence. It may of course in turn produce other motions in the calcium, as he suggests. If, however, the latter were those he observes, we should scarcely expect them to be communicated to the hydrogen, which is about a million-fold more abundant and is apparently not affected by radiation-pressure.* On the contrary, the "granulations" are also observed in hydrogen light, showing that the hydrogen is actually in turbulent motion. Further, Dr. Unsöld kindly informs me by letter that he has made preliminary measures of the contours of the hydrogen lines H_a and H_ϵ and the Ca^+ lines H and K in a prominence, and finds them apparently determined by Doppler effects as in the Ca^+ chromosphere and with velocities of the same order as in the chromosphere. This indicates that he is observing true turbulence, in which of course all the elements must have the same velocities, rather than some individual behaviour of calcium. (Measures for hydrogen in the normal chromosphere do not appear to exist.)

All that matters for us at the moment is, however, merely the observational fact that the hydrogen chromosphere is in turbulent motion, and that the mean velocity of the turbulence is probably that given by Unsöld's measurements for Ca^+.

3. *The Pressure in a Turbulent Gas.*—It seems quite evident physically that for the dynamical pressure in a gas it does not matter whether the atoms move quite independently with given velocities (pure temperature motion) or whether they move in groups or clusters with those velocities (turbulent motion), provided, of course, the dimensions of the groups are sufficiently small compared with other dimensions involved.

We shall attempt, however, to give the ideas mathematical form.

The first thing is to define our turbulent motion.

We suppose that the gas in any region at any instant can be divided up into volume elements (*of the gas*, not of the space) of finite extent Δ, say, such that for all but a small fraction of them the following properties are true: (*a*) Each element contains a sufficiently large number of particles. (*b*) It is a good approximation to regard the motions of the molecules † inside any element relative to the element as completely chaotic, *i.e.* any relative mass motions inside the element are small compared with the individual motions of the molecules. This chaotic motion can then be described by a temperature T which we assume to be the same for all types of molecule present. We denote by u, v, w the velocities of a molecule relative to the element. (*c*) A volume element as a whole has velocity components U, V, W defining the turbulent motion. We shall suppose the turbulence has a Maxwellian distribution, *i.e.* the number of elements with U-velocities in the range U, U + dU is proportional to $\exp\left(-\frac{U^2}{2\overline{U^2}}\right)$, where $\overline{U^2}$ is the mean of U^2. This supposes the gas to have no mass motion as a whole.

* See McCrea, *M.N.*, **89**, 483 (1929): The factor "a million" is from my estimate for ionised hydrogen and Unsöld's for Ca^+, and refers to the number of atoms per cm.^{3}.

† "Molecules" in the wide sense.

If some such definition does not apply, then it is impossible to distinguish between the temperature motion and the turbulence which, in that case, must actually have been largely transformed to heat.

If, however, it is possible to speak of a temperature defined by molecular motion apart from the turbulence, then such a definition must hold. The object of separating the motions is that if there is this temperature it will be the same for all types of molecule present, and so will give each type a different mean velocity. The turbulent velocities U, V, W, on the other hand, are the same for all molecular types.

It would seem that the definition does apply, at least approximately, to the chromosphere. At any rate we suppose so in §§ 5, 6, and work out the consequences, and then discuss the alternatives.

The pressure in such a gas, averaged over an area large compared with the dimensions of the above volume elements, is easily found. For consider first the molecules of type j. Let there be n_j such per unit volume, each of mass m_j. Let $\mathbf{U}$, $\mathbf{V}$, $\mathbf{W}$ be the resultant velocity components of one such molecule. Then $\mathbf{U} = \mathrm{U} + u$, etc. The pressure p_j due to these molecules is the same in all directions. By precisely the usual arguments it is given for the x-direction by *

$$p_j = n_j m_j \overline{\mathbf{U}^2} \quad . \quad . \quad . \quad . \quad (1)$$

Now

$$\overline{\mathbf{U}^2} = \overline{(u + \mathrm{U})^2} = \overline{u^2} + \overline{2u\mathrm{U}} + \overline{\mathrm{U}^2} = \overline{u^2} + \overline{\mathrm{U}^2},$$

since by symmetry the mean of $u\mathrm{U}$ must vanish.

But $n_j m_j \overline{u^2} = n_j k\mathrm{T}$ in the usual way.

Hence

$$p_j = n_j k\mathrm{T} + n_j m_j \overline{\mathrm{U}^2} \quad . \quad . \quad . \quad (2)$$

Clearly by symmetry we have

$$\overline{\mathrm{U}^2} = \tfrac{1}{3}(\overline{\mathrm{U}^2} + \overline{\mathrm{V}^2} + \overline{\mathrm{W}^2}) = \tfrac{1}{3}\mathrm{C}^2 \text{ (say)} \quad . \quad . \quad (3)$$

The simplest expression of the result is obtained by defining an "apparent temperature" $\mathbf{T}_j$ for each molecular type by the relation

$$k\mathbf{T}_j = k\mathrm{T} + \tfrac{1}{3}m_j\mathrm{C}^2 \quad . \quad . \quad . \quad . \quad (4)$$

Substituting in (2) and summing over all types, the total pressure is

$$p = \sum_j p_j = \sum_j n_j k_j \mathbf{T}_j \quad . \quad . \quad . \quad (5)$$

We can apply this to the case of gravitational equilibrium.

If there are only different types of neutral atoms present each will obey a density law corresponding to its own m_j and $\mathbf{T}_j$. If turbulence is negligible we have the usual Dalton's law. If it is predominant so that $\frac{1}{3}m_j\mathrm{C}^2 >> k\mathrm{T}$ for all m_j, then the gas will have a uniform composition throughout.

* Jeans, *Dyn. Theory of Gases* (1925), p. 116, or by virial method, p. 131.

We shall also need the case where one constituent, suffix 1 say, is ionised. The work of Milne * and Fowler † cannot be extended immediately to the present case. If we attempt to use it we find the following results.

By their method the un-ionised part of the gas would behave as before and so possess a density gradient of the form

$$n_1 \propto \exp\left(-\frac{m_1 g y}{k\mathbf{T}_1}\right) \quad . \quad . \quad . \quad . \quad (6)$$

For the ions of mass m_i and electrons of mass m_e an electrostatic potential $\left(\frac{\mathrm{X}}{e}\right)$ would be set up, and we should have density laws of the respective forms

$$n_i \propto \exp\left(\frac{-m_i g y + \mathrm{X}}{k\mathbf{T}_i}\right) \quad . \quad . \quad . \quad (7)$$

$$n_e \propto \exp\left(\frac{-m_e g y - \mathrm{X}}{k\mathbf{T}_e}\right) \quad . \quad . \quad . \quad (8)$$

Here $\mathbf{T}_i$, $\mathbf{T}_e$ are determined by equations like (4). But $m_e << m_i$, so that $m_i \cong m_1$ and hence $\mathbf{T}_i \cong \mathbf{T}_1$, but $\mathbf{T}_e \neq \mathbf{T}_i$. (In general we shall have $\frac{1}{3}m_e \mathrm{C}^2 << k\mathrm{T}$, so that $\mathbf{T}_e \cong \mathrm{T}$.)

From the work of Milne and Fowler and others we have the well-known general physical result that the ions and electrons do not appreciably separate out under gravity. Applying this here, the gradients (7), (8) must be to a high approximation equal. Writing down this condition and noting that for all ordinary cases $\frac{m_e}{\mathbf{T}_e}$ can be neglected in comparison with $\frac{m_i}{\mathbf{T}_i}$, we would obtain

$$n_i = n_e \propto \exp\left\{-\frac{m_i g y}{k(\mathbf{T}_i + \mathbf{T}_e)}\right\} . \quad . \quad . \quad (9)$$

Hence the density gradient for ions and electrons would be the same as for neutral atoms at temperature $\mathbf{T}_i + \mathbf{T}_e$.

If there is no turbulence we have $\mathbf{T}_i = \mathbf{T}_e = \mathbf{T}_1 = \mathrm{T}$, and so we get back to the result given by the authors quoted.

But consider the general case. Whatever factor controls the degree of ionisation we must expect to have the general form of the ionisation formula obeyed, viz.

$$n_1 = c n_i^2 = c n_e^2 \quad . \quad . \quad . \quad . \quad (10)$$

where c is a constant, since we are supposing everything but pressure to remain constant throughout the gas. Applying (10) to (6) we should have

$$n_i \propto \sqrt{n_1} \propto \exp\left(-\frac{m_1 g y}{2k\mathbf{T}_1}\right) \quad . \quad . \quad . \quad (11)$$

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* Milne, *Proc. Camb. Phil. Soc.*, **22**, 493 (1925).

† Fowler, *Statistical Mechanics* (1929), p. 359 ff.

which contradicts (9), since $\mathbf{T}_i + \mathbf{T}_e \neq 2\mathbf{T}_1$. Equations (6) and (9) together would mean that the ionisation formula could not hold for more than one y-level in the gas, which would be ridiculous.

The reason for the discrepancy appears to be that the previous methods are physically inadequate to deal with this case in that they treat the neutral and ionised atoms too independently. The calculations originate in the consideration of an element of volume, and in expressing the condition that the change of momentum (pressures) in unit time is equal to the forces acting. Now this condition does not strictly hold for the neutral atoms and positive ions independently. For each atomic nucleus spends part of its time in an atom and part as an ion, and it is only reasonable to consider all the nuclei together and speak of the average force on them over a sufficiently long time. Similarly for the electrons. If we follow this method we lose an equation as compared with former methods. But this is supplied by making the natural demand that the ionisation equation must be satisfied throughout the gas. This is equivalent to giving the fraction of time spent in each state, ionised and un-ionised.

We express the ideas mathematically by considering an elementary cylinder of length dy in the y-direction and unit cross-section. Consider first the positive nuclei in the element. The increase in momentum in unit time in the y-direction is

$$-(dp_1 + dp_i) = -(k\mathbf{T}_1 dn_1 + k\mathbf{T}_i dn_i) = -k\mathbf{T}_i d(n_1 + n_i) \quad (12)$$

since $\mathbf{T}_i = \mathbf{T}_1$. The force due to gravity is $-m_1(n_1 + n_i)gdy$. The mean electrical force is $n_i \mathrm{X} dy$. Putting these together we obtain

$$k\mathbf{T}_i d(n_1 + n_i) = -m_1(n_1 + n_i)gdy + n_i \mathrm{X} dy \,. \quad (13)$$

Consider next the electrons in the volume element. Owing to their small mass, those moving with nuclei in the form of atoms will contribute very little to the momentum. The change per unit time is therefore $dp_e = k\mathbf{T}_e dn_e$. The force due to gravity may be neglected. The mean electrical force is $-n_e \mathrm{X} dy$. Hence we find

$$k\mathbf{T}_e dn_e = -n_e \mathrm{X} dy \,. \quad (14)$$

Our extra condition is the ionisation equation (10), where c is supposed known from the conditions of ionisation and recombination. We also know that we must have as a good approximation

$$n_i = n_e \,. \quad (15)$$

since otherwise the charges would separate. Using this and adding (13) and (14) we find

$$k\mathbf{T}_1 dn_1 + k(\mathbf{T}_i + \mathbf{T}_e)dn_i = -m_1(n_1 + n_i)gdy \quad (16)$$

Substituting from (10) this can be put in the form

$$\{2ck\mathbf{T}_1 n_i + k(\mathbf{T}_i + \mathbf{T}_e)\}dn_i = -m_1 n_i(cn_i + 1)gdy \quad (17)$$

This clearly does *not* in general allow of (6) or (9) as a solution. We

shall not, however, look for the general solution but consider two special cases.

From (10) we have

$$\frac{dn_1}{n_1} = 2\frac{dn_i}{n_i} \quad . \quad . \quad . \quad . \quad (18)$$

so that if

$$n_i << n_1 \text{ then } dn_i << dn_1 \text{ and conversely} \quad . \quad . \quad (19)$$

The cases are:

(1) Low ionisation, $n_i << n_1$. Then from (16) and (19) we have approximately

$$k\mathbf{T}_1 dn_1 = -m_1 n_1 g dy,$$

so that in this limiting case the usual result (6) holds good, but the corresponding n_i, n_e must be calculated from (10) and not (9).

(2) High ionisation, $n_i >> n_1$. Here we have approximately from (16) and (19),

$$k(\mathbf{T}_i + \mathbf{T}_e)dn_i = -m_1 n_i g dy.$$

Hence in this limiting case the result (9) holds, but the corresponding n_1 must come from (10) and not (6).

We thus have the natural results that when one fraction is large it obeys a density law as if it alone were present, and then the smaller fraction can be calculated from it according to the ionisation formula.

This point has been treated at some length, but it is needed in § 5 and also seems to have some interest of its own in connection with the equilibrium of an ionised gas.

4. *The Value of C.*—We shall derive this from the quoted work of Unsöld. He found the turbulent velocity for Ca^+ large compared with its temperature motion, and so we shall make the approximation of neglecting the latter in this case. His line contours were given by the Doppler effect. For this only the velocities ω, say, in the line of sight matter. He took a Maxwellian distribution in the form * $e^{-\left(\frac{\omega}{\omega_0}\right)^2}$ for these velocities and found ω_0 to be 15 km./sec. We see from this that his ω_0^2 is precisely our $2\overline{U^2}$, so that we obtain at once from his work

$$C^2 = 3\overline{U^2} = \tfrac{3}{2}\omega_0^2; \qquad C = 18{\cdot}4 \text{ km./sec.} \quad . \quad . \quad (20)$$

5. *The Hydrogen Chromosphere.*—We suppose the theory of § 3 applies. The temperature T then exists and we shall take it to be that usually assumed for this region, namely, T = 5000°. From (4) with C from (20) we obtain immediately for the values appropriate to hydrogen,

$$\mathbf{T}_1 = \mathbf{T}_i = 18630°,$$
$$\mathbf{T}_e = 5007°,$$

the suffixes referring to the neutral atom, ion, and electron respectively.

For comparison we may mention that the mean kinetic energy of hydrogen atoms at 5000° corresponds to 11·13 km./sec. This is no longer small compared with the turbulence. It is therefore really

* Unsöld, *loc. cit.*, equation (6).

fortunate that the turbulent velocity was derived from a heavier atom, so that there was no trouble about trying to separate out the temperature motion. We may also mention that for Ca^+ we have $\mathbf{T}_{Ca^+} = 550{,}200°$.

We emphasise that these $\mathbf{T}$ are analogous to temperatures only for calculating average pressures. Degrees of excitation and ionisation will be expected * to correspond to about 5000°, as do the more detailed motions of the atoms amongst themselves.

Since the hydrogen is present in such an excess we may consider its equilibrium as though it alone were present.

If the hydrogen is largely neutral, then we have at once from § 3 a density gradient corresponding to $\mathbf{T}_1 = 18{,}630°$, which can be written

$$n_1 \propto \exp\left(-1237\,\frac{y}{a}\right). \qquad . \qquad . \qquad . \qquad (21)$$

where a is the solar radius. If it is largely ionised, then by § 4 the ions possess a density gradient corresponding to $\mathbf{T}_i + \mathbf{T}_e = 23{,}637°$, and consequently from (10) the neutral atoms have one corresponding to $\frac{1}{2}(\mathbf{T}_i + \mathbf{T}_e) = 11{,}819°$, so that

$$n_1 \propto \exp\left(-1949\,\frac{y}{a}\right). \qquad . \qquad . \qquad . \qquad (22)$$

Now the observations of Pannekoek and Minnaert † give directly the number of 5-quantum atoms which we may suppose to be a constant fraction of the whole number of neutral atoms, and so lead to the observed result

$$n_1 \propto \exp\left(-1127\,\frac{y}{a}\right). \qquad . \qquad . \qquad . \qquad (23)$$

A comparison of (23) with (21) or (22) is most satisfactory. It shows at once that the observed amount of turbulence gives quite the right order of density gradient in either case. No previous attempt gave anything but a gradient of the order $n_1 \propto \exp\left(-4600\,\frac{y}{a}\right)$. What is more, the observed turbulence apparently *must* give such gradients as (21) or (22). If it is real the value $\exp\left(-4600\,\frac{y}{a}\right)$ is impossible.

It is worth going into more detail. In *M.N.* (*loc. cit.*) I showed that if the number of normal hydrogen atoms in the chromosphere is subject to no anomaly then observation apparently indicates that the gas is actually only about 2 per cent. ionised. Then (21) really would apply, so that the theoretical value agrees with the observational (23) probably well within the errors of observation. Further, a difference of 10 per cent. in the observed ω_0 would mean one of about 20 per cent.

* See McCrea, *Proc. Camb. Phil. Soc.*, **24**, 506 (1928); *M.N.*, **89**, 483 (1929); *Nature*, **123**, 527 (1929).

† Pannekoek and Minnaert, *Verh. d. kon. Akad. Amsterdam*, **13**, No. 5 (1928); see also McCrea, *M.N., loc. cit.*

in the computed coefficient of $\frac{y}{a}$ in (21). It is therefore very striking that Unsöld's observed ω_0 leads so exactly, without any pressing of the data, to the right density law for hydrogen.

The anomaly affecting the number of normal hydrogen atoms in the reversing layer is well known—the computed number always comes out much too large.* If this were to hold good in the chromosphere (a very difficult thing to verify owing to the impossibility of observing any quantity directly connected with the normal atoms) we should probably have the case of high ionisation leading to (22). The coefficient 1949 as it stands is then too large. But a change from C = 18·4 to 28 km./sec. would give complete agreement. This change is not unthinkable considering how few observations are available. Hence the precise numerical agreement of the last paragraph may not be considered conclusive until more observations are made and until more is known about the puzzling behaviour of the normal atoms. But the consideration of this most unfavourable case shows that the turbulence is responsible for such a large decrease in the density gradient compared to that without turbulence, that one cannot help suggesting that it must probably be in some way responsible for the whole of it.

It is important to remember that Unsöld's velocity is explicitly only a mean for the whole depth of the chromosphere. Variations in it would be much more important here than for his purpose. It is also important to notice that he records considerable variation in different parts of the sun.

It is only natural to suppose that the turbulence, however it arises, will not be produced uniformly throughout the gas, but that in some regions it will initially correspond to a different **T** from that in others. It is possible that the gas from the different regions then mixes so quickly that we may only speak of one mean **T** for the whole gas. But those parts with initially higher **T** will always have the tendency to spread out higher than those with lower **T**. This may be aided or hindered by the precise way in which the turbulence is generated. But if the mixing does not altogether overcome such a tendency, we may expect to find a higher **T**, and so a smaller density gradient, for higher layers. This must be in reality the case if turbulence does play the part we are suggesting. For the density gradient found by Pannekoek and Minnaert is too great to hold for all heights, and in fact they do actually find a tendency for it to decrease at the greatest heights, about 3000 km., included in their measurements.

6. *Other Elements.*—The question of the effect of the turbulence on other elements at once presents itself.

We consider first the Ca^+. Now the calcium present is practically entirely once ionised to Ca^+, but is apparently in the presence of an excess of electrons from the hydrogen, so that the investigation of § 3 does not immediately apply. Judging by Fowler's work (*loc. cit.*) on the analogous problems without turbulence, the present case would be still more complicated. But if § 3 may be taken as a qualitative

* See, for example, Unsöld, *Zs. f. Phys.*, **46**, 780 (1928).

indication of what happens we expect the density law to correspond to something between $\mathbf{T}_{Ca^+}$ and $\mathbf{T}_{Ca^+} + \mathbf{T}_e$. These however are not very different for Ca^+, so it is sufficient to say that the law is approximately that for $\mathbf{T}_{Ca^+}$. This we have seen to be 550,200° for our former C. It leads to a gradient, $\exp\left(-1674\,\frac{y}{a}\right)$. But on account of what has been said at the end of § 5 we must conclude that it would be less at great heights. It would be just a little more than that of the hydrogen if the latter is not highly ionised, and a little less than that of the neutral hydrogen if it is largely ionised. If observation is ultimately found approximately to verify such a relation we should conclude that turbulence is also an important factor in the support of the Ca^+. If, however, the latter were found to have a much smaller density gradient we should conclude that radiation-pressure alone is the determining factor, as in Milne's theory.

The real interest for other elements is that if the theory is correct it explains how they occur at all in the chromosphere. For most elements we shall have $\frac{k\mathbf{T}_j}{m_j} \cong \frac{1}{3}C^2$, since the temperature motion will be small compared with C. So, to a first approximation, we should expect all elements to have the same density gradient. This will be about the order of that just calculated for Ca^+ in the absence of radiation-pressure. It does not of course follow that they will all be observed up to the same heights, because they will not be equally abundant and all of them probably less abundant than hydrogen. Some of them will also become further ionised at high levels and so cease to give the same spectral lines as lower down.

It has long been a mystery how these elements rise so high in the atmosphere. The importance of radiation-pressure for Ca^+ seemed scarcely to admit of generalisation. Also I showed (*M.N.*, *loc. cit.*), I believe correctly, that even that importance was dependent on the presence of enough hydrogen to prevent excessive ionisation. It therefore appears very satisfactory to have found such an agency as turbulence which seems to explain the presence of these elements without appeal to any atomic peculiarities, not even their lightness.

7. *The Definition of Turbulence.*—It seems certain that there is no other possibility but that the definition of § 3 applies to the motion of the chromosphere. It must be possible to separate out most of the observed motion of the Ca^+ as turbulent motion distinct from temperature motion. Otherwise the temperature would be impossibly high. There is just the possibility that *some* fraction of the turbulent motion has actually degenerated into heat motion. But this would then hold for the hydrogen as well, and if it appreciably raised the temperature would disturb my former estimates of the degree of ionisation. These, however, seemed satisfactory as they stood.

The reasonable results arrived at in the preceding paragraphs further strengthen this view.

8. *Further Behaviour of the Turbulence.*—Our main object has been to start with the turbulence known to be present and to examine its

effect on the pressure, not to give a theory of the turbulence. Its existence gives rise, however, to many other questions, and it may be permissible to offer a few preliminary considerations.

In the first place, why does the turbulence apparently remain constant and not become transformed to pure temperature motion? We might suppose a steady state is attained in which as much turbulence is transformed into heat as heat is radiated away per unit time. This is not very satisfactory, for it would then be hard to see why the temperature keeps roughly the value we expect without turbulence and to see how the turbulence could be maintained through the body of the gas.

A more natural way is to suppose the gas which forms the chromosphere is continually being changed by some process which ensures that when any portion of gas is in the chromosphere its turbulent motion is always at approximately the same stage.

Two rather different possibilities suggest themselves. The first is that the currents, whatever they may be, which give rise to the turbulence by a transition from laminar motion, retain enough control over the chromospheric gases to keep them in circulation. The second arises out of the following consideration. If the "apparent temperature" **T** decreases inwards in any region there will be a conduction inwards of the energy of turbulent motion, exactly analogous to the ordinary conduction of heat by a transference of purely temperature motion. This would naturally cause the outer parts to contract. Put another way, suppose there be somewhere a horizontal plane of discontinuity so that the turbulent motion on the lower side is less than on the upper. Then such a surface will not be perfectly reflecting for the gas above it. The latter will therefore not behave as if resting on a solid surface, but must gradually sink downwards. This holds good when we replace the plane of discontinuity by a region of gradual weakening of the turbulence.

Now we have already seen occasion to suppose that the turbulence does increase as we go to greater heights in the chromosphere. It is further very possible that it becomes much weaker in the reversing layer. This then necessitates a gradual settling down of the chromospheric gases. It is remarkable that this is just what is demanded by the red-shift of chromospheric lines observed by St. John.* It seems consistent with the characteristic properties of this shift as being correlated with height and not with line intensity or apparently with the element concerned. The suggested explanation can scarcely be considered as *ad hoc*. It seems to follow naturally from the mere presence of rather violent turbulence in the upper layers which we cannot suppose maintained to the same extent in the lower. The term "gradual" might be objected to as a description of the observed velocity of about 1 km./sec. All that is intended is merely something probably appreciably slower than free fall under gravity.

We come upon the same problem, of course, as do St. John and Babcock † as to how the descending gas can be replaced. As in their

* St. John, *Proc. Nat. Acad. Sci.*, **12**, 65 (1926).
† St. John and Babcock, *Ap. J.*, **60**, 32 (1924).

discussion the action of prominences suggests itself but with a wider significance. They liken the spreading out of the prominence matter at high levels to the spreading out of volcanic dust in the terrestrial atmosphere. But Rosseland's considerations quoted before suggest another possibility. Suppose a prominence rises with a velocity of 20 km./sec. According to Rosseland the motion will become transformed into turbulence, and observation suggests that this happens at great heights. The atoms, however, must retain their kinetic energy except in so far as it is changed into potential energy, but this will be recovered, or in so far as it is communicated to the surrounding atmosphere, and this is not likely to be very important as the prominence must be much denser than the atmosphere. So the "apparent temperature" **T** of the turbulence generated corresponds to a mean velocity about 20 km./sec. From § 3 it is this "temperature" which controls the pressure, and so the turbulent matter will spread out just like a gas at temperature **T**. This, however, is precisely what is wanted to supply a turbulent atmosphere of the kind observed. So if the suggestion, which, be it repeated, is yet only tentative, proves valid, it not only explains how the matter is brought up to the high levels but also how it attains its turbulence. In addition, a variety of prominence velocities would give a variety of **T** values and so account for the change of density gradient with height discussed before.

Velocities of the right order seem to be quite common in prominences. * The average for *eruptive* prominences is much greater, and this seems to be important in the problem of the corona. It is therefore suggested that the relatively more slowly moving prominences may play an essential part in supplying material and motion to the chromosphere. The same result might also be contributed to by the rising currents over bright granules suggested by St. John and Babcock.

Observations on any correlation between prominence activity and the height of the chromosphere would be very valuable in this connection.

9. *The Effect on the Reversing Layer.*—It is generally assumed † that it is a good approximation to take the reversing layer as in gravitational equilibrium at a temperature about 5000°. This would have to be modified if the considerations of this paper hold good in this layer. The extent to which they do so is not yet known.

10. *Further Applications.*—It appears that turbulence may also be of considerable importance in other connections. A certain amount of success has been obtained by the writer with a somewhat similar theory of the corona. Further, once the motion of a gas has become turbulent it must ultimately degenerate into heat. The thermodynamic state must therefore be affected. Qualitatively the effect would appear to show the same characteristics as the observed ‡ deviations from thermodynamic equilibrium in stellar atmospheres.

* Abetti, *Handb. d. Astrophys.*, Bd. iv. (1929); Pettit, *Publ. Yerkes Obs.*, **3**, pt. iv. (1925).

† See, for example, Milne, *M.N.*, **89**, 18 (1928).

‡ Adams and Russell, *Ap. J.*, **68**, 9 (1928).

It is hoped on a later occasion to give accounts of investigations of both these points.

Summary.

1. The work is an attempt to allow for the effect of turbulence on the gas-pressure in the solar chromosphere.

2. The failure of former attempts to explain the pressure gradient is noted.

3. It is pointed out that Rosseland has shown that turbulence is necessarily present, and that it has been found experimentally by Unsöld working on the chromospheric lines of Ca^+.

4. If C^2 is the mean square velocity of the turbulence and T the temperature of the gas, then it is possible to define an "apparent temperature" $\mathbf{T}_j = T + \frac{1}{3}m_j C^2$ for each molecular type j. The pressure gradient under gravity will in general correspond to $\mathbf{T}_j$, not T.

5. It is shown how the last result must be modified for an ionised gas.

6. The velocity C as derived from Unsöld's results is 18·4 km./sec.

7. This is used in a numerical application of the theory to hydrogen. The pressure gradient for the neutral atoms is $\exp\left(-1237\frac{y}{a}\right)$ or $\exp\left(-1949\frac{y}{a}\right)$ according as the degree of ionisation is small or large. The observations of Pannekoek and Minnaert give $\exp\left(-1127\frac{y}{a}\right)$. Former theory gave always about $\exp\left(-4600\frac{y}{a}\right)$. It is difficult to decide which case to apply, but at the present stage either of the new results may be regarded as satisfactory.

8. Apart from difficulties due to ionisation, the theory gives density gradients for all elements of approximately the same order as for hydrogen, and so explains how they penetrate into the chromosphere.

9. The turbulence does not appear to degenerate into heat motion in the chromosphere.

10. It is pointed out that this probably means that the gas forming the chromosphere is continually being changed. It is further seen that a turbulent atmosphere probably finds insufficient support from below to remain permanently in position. It will gradually sink down. A connection with the downward motion observed by St. John is suggested. It is shown how prominences might be the origin of the turbulence and also maintain the supply of material.

11. The mechanical equilibrium of the corona and the mechanical and thermodynamic equilibrium of the reversing layer may also be influenced by turbulence. It is hoped to discuss this later.

Göttingen:
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The Early History of the Solar System on the Collision Theory.

By Harold Jeffreys, D.Sc., F.R.S.

1.0. In investigations of the early history of the planets and satellites on the tidal theory of their origin, there has always been an uncertainty arising from lack of knowledge of the depth within the sun where the ejected matter can be supposed to have been before the encounter. In a slow encounter it would necessarily be superficial matter, but in a transitory or intermediate encounter it might have come from a considerable depth. There is also some doubt as to the nature of the thermal changes during the process. Hitherto I have worked on the hypothesis that the ejected matter had a density comparable with the mean density of the sun or a tenth of it.* It appears in the present paper that this requires considerable modification.

1.1. On the theory of material collision, the collision must have deformed the sun by a moderate fraction of its radius. The whole process of collision and ejection occupied a time of the order of an hour, and it is reasonable to treat the process as adiabatic. We may still consider two possibilities: first, that the ejected matter actually came from a moderate depth in the sun originally; second, that it was originally near the surface, and was merely pressed inwards by the collision, without mixing with other solar or stellar matter and without transfer of heat to any important extent. The latter possibility is more probable, but it is desirable to investigate both.

We suppose that the ejected matter followed the law

$$p = k\rho^{\gamma} . \quad . \quad . \quad . \quad . \quad (1)$$

where p is the pressure, ρ the density, γ the ratio of the specific heats, and k a constant determined by the conditions before ejection. Also

$$p = \mathrm{R}\rho\mathrm{T} \quad . \quad . \quad . \quad . \quad (2)$$

where T is the temperature and R a constant for the material. The pressure within the planet satisfied the equation

$$dp = \rho d\mathrm{U} \quad . \quad . \quad . \quad . \quad (3)$$

where U is the gravitation potential. Since p vanishes at the surface, the mean value of p within the mass is of the order of magnitude of the product of the mean density and the difference between the values of U at the centre and the surface. The latter is of the order of fm/a, where f is the constant of gravitation, m the total mass, and a the radius. The mean pressure is therefore of the order of $f\rho m/a$; or, since

$$m = \tfrac{4}{3}\pi\rho a^3 . \quad . \quad . \quad . \quad . \quad (4)$$

$$p = O(4f\rho^2 a^2) \quad . \quad . \quad . \quad . \quad (5)$$

* *The Earth*, 1929, pp. 26–30.

Hence by (1) $$4f\rho^{2-\gamma}a^2 = O(k) \quad . \quad . \quad . \quad . \quad (6)$$

Also

$$\mathrm{T} = \frac{k}{\mathrm{R}}\rho^{\gamma-1} \quad . \quad . \quad . \quad . \quad (7)$$

The values of ρa^3 for the planet and sun are in the ratio of the masses. We use suffix p for the planet, s for the sun.

1.2. If the ejected matter came from a moderate depth in the sun, k was the value of p/ρ^γ before ejection, and hence is of the order of $4f{\rho_s}^{2-\gamma}{a_s}^2$. Thus

$$\left(\frac{\rho_p}{\rho_s}\right)^{2-\gamma}\left(\frac{a_p}{a_s}\right)^2 = O(1) \quad . \quad . \quad . \quad (1)$$

But

$$\frac{\rho_p}{\rho_s}\left(\frac{a_p}{a_s}\right)^3 = \frac{m_p}{m_s} \quad . \quad . \quad . \quad . \quad (2)$$

Eliminating ρ_p/ρ_s we have

$$\frac{a_p}{a_s} = O\left(\frac{m_p}{m_s}\right)^{\frac{2-\gamma}{4-3\gamma}} \quad . \quad . \quad . \quad . \quad (3)$$

Also

$$\frac{\rho_p}{\rho_s} = O\left(\frac{a_p}{a_s}\right)^{-\frac{2}{2-\gamma}} = O\left(\frac{m_p}{m_s}\right)^{-\frac{2}{4-3\gamma}} \quad . \quad . \quad (4)$$

$$\frac{\mathrm{T}_p}{\mathrm{T}_s} = O\left(\frac{m_p}{m_s}\right)^{-\frac{2(\gamma-1)}{4-3\gamma}} \quad . \quad . \quad . \quad . \quad (5)$$

The radius of the planet when formed would increase with its mass if $\dfrac{2-\gamma}{4-3\gamma}$ was positive, not otherwise, and it is therefore important to know whether γ is greater or less than $\frac{4}{3}$. This condition also determines the stability of a gaseous mass. A gaseous planet has a period of vibration, for radial displacements,* of $2\pi\{(3\gamma-4)f\rho\}^{-\frac{1}{2}}$. This is imaginary if $\gamma<\frac{4}{3}$, and shows that such a planet is radially unstable. It would either spread out till it filled all space or collapse to its centre, according as its original radius was greater or less than the equilibrium value for the mass. But originally the matter was fresh from a moderate depth in the sun, and $\rho_p/\rho_s = O(1)$. Hence by (2) the original value of a_p/a_s was of order $(m_p/m_s)^{\frac{1}{3}}$, and this is greater than the equilibrium value given by (3) if $\frac{1}{3}$ is greater than $\dfrac{2-\gamma}{4-3\gamma}$, since $\dfrac{m_p}{m_s}$ is in any case less than 1. But

$$\frac{1}{3} - \frac{2-\gamma}{4-3\gamma} = -\frac{2}{3(4-3\gamma)}$$

and is negative if $\gamma<\frac{4}{3}$. Thus if $\gamma<\frac{4}{3}$ the original radius was greater than the equilibrium radius, and the planet would expand to fill all space.

* Ritter; quoted by Jeans, *Problems of Cosmogony*, p. 190.

For a monatomic gas

$$\gamma = \tfrac{5}{3}; \qquad \frac{2-\gamma}{3\gamma-4} = \frac{1}{3}.$$

Stable equilibrium is now possible. The primitive radius of Jupiter would be of the order of ten times the radius of the sun, say, 7×10^6 km. This is rather less than the mean distance of its sixth satellite. The corresponding amount for Saturn is 10^7 km., about the distance of Phœbe.

But (5) gives

$$\frac{T_p}{T_s} = O\left(\frac{m_p}{m_s}\right)^{\frac{1}{3}},$$

and the average absolute temperature in Jupiter was of the order of 10^{-4} of that in the sun. The sun, according to Eddington,* has a central temperature of 4×10^7 degrees. The temperature where the density has its mean value, according to the table on p. 83 of his book, is about $\frac{1}{4}$ of this. Thus the average temperature in Jupiter would be about 1000° absolute. The density would be about 10^{-6} gm./cm.^{3}. Hence silicon and heavy metals would probably liquefy ; most elements would form compounds. On both grounds the gaseous equilibrium state would not be reached. For smaller planets the changes are of course greater.

For a diatomic gas

$$\gamma = \tfrac{7}{5}; \qquad \frac{2-\gamma}{3\gamma-4} = 3; \qquad \frac{2(\gamma-1)}{3\gamma-4} = 4.$$

The equilibrium radius of Jupiter would be 10^9 times that of the sun. This is 7×10^{14} km., or 10^5 times the distance of Neptune. The distension is so enormous that further consideration is unnecessary.

1.3. On the alternative hypothesis a depth corresponding to about $\frac{1}{500}$ of the mass of the sun was swept off the outside. In evaluating k from the conditions before ejection we can no longer use mean values of p and ρ for the sun. But

$$k = p/\rho^\gamma$$
$$= (p/\rho^{\frac{4}{3}})^{\frac{3}{4}\gamma} p^{1-\frac{3}{4}\gamma} \quad . \quad . \quad . \quad . \quad (1)$$

all quantities being evaluated for the outermost $\frac{1}{500}$ of the sun's mass. But in a gaseous star $p/\rho^{\frac{4}{3}}$ is constant and can be given a mean value, while p is here about $\frac{1}{500}$ of the mean pressure, since radiation-pressure within the sun is much less than hydrostatic pressure. Hence

$$k = O\{500^{\frac{3}{4}\gamma-1}(p_s/\rho_s{}^\gamma)\} \quad . \quad . \quad . \quad . \quad (2)$$
$$= O\{500^{\frac{3}{4}\gamma-1} \cdot 4f\rho_s{}^{2-\gamma}a_s{}^2\} \quad . \quad . \quad . \quad (3)$$

Combining this with 1.1 (6) we have

$$\left(\frac{\rho_p}{\rho_s}\right)^{2-\gamma}\left(\frac{a_p}{a_s}\right)^2 = O\left(500^{\frac{3}{4}\gamma-1}\right) \quad . \quad . \quad . \quad (4)$$

* *The Internal Constitution of the Stars*, p. 149.

while as before

$$\frac{\rho_p}{\rho_s}\left(\frac{a_p}{a_s}\right)^3 = \frac{m_p}{m_s} \quad . \quad . \quad . \quad . \quad (5)$$

Eliminating ρ_p/ρ_s we have now

$$\frac{a_p}{a_s} = O\left\{500^{\frac{1}{4}}\left(\frac{m_p}{m_s}\right)^{\frac{2-\gamma}{4-3\gamma}}\right\} \quad . \quad . \quad . \quad (6)$$

and the equilibrium radii are about five times as great as on the other hypothesis. Also

$$\frac{\rho_p}{\rho_s} = O\left\{500^{-\frac{3}{4}}\left(\frac{m_p}{m_s}\right)^{-\frac{2}{4-3\gamma}}\right\} \quad . \quad . \quad . \quad (7)$$

If T_o is the temperature of the matter before ejection,

$$T_p = T_o\left(\frac{\rho_p}{\rho_o}\right)^{\gamma-1} \quad . \quad . \quad . \quad . \quad (8)$$

while

$$\left(\frac{\rho_o}{\rho_s}\right)^{\frac{4}{3}} = \frac{p_o}{p_s} = O\left(\tfrac{1}{500}\right) \quad . \quad . \quad . \quad (9)$$

Thus

$$\frac{\rho_p}{\rho_o} = \frac{\rho_p}{\rho_s}\Big/\frac{\rho_o}{\rho_s} = O\left\{500^{-\frac{3}{4}}\left(\frac{m_p}{m_s}\right)^{-\frac{2}{4-3\gamma}}\right\}\Big/(500)^{-\frac{3}{4}} \quad . \quad (10)$$

$$= O\left(\frac{m_p}{m_s}\right)^{-\frac{2}{4-3\gamma}}$$

Also

$$\frac{T_o}{T_s} = \left(\frac{\rho_o}{\rho_s}\right)^{\frac{1}{3}} = 500^{-\frac{1}{4}} \quad . \quad . \quad . \quad (11)$$

and

$$T_p = O\{500^{-\frac{1}{4}}T_s\}\left(\frac{m_p}{m_s}\right)^{-\frac{2(\gamma-1)}{4-3\gamma}} \quad . \quad . \quad . \quad (12)$$

The temperatures are therefore also lower than on the other hypothesis.

1.4. In general our result is that the equilibrium densities are so low that values obtained on previous working hypotheses call for considerable change. The temperatures imply that the more refractory monatomic materials, such as silicon, would certainly, others possibly, liquefy before gaseous equilibrium was reached. For diatomic and more complex materials it seems that the matter would spread out to such a distance that liquefaction would certainly occur before equilibrium. The process would involve the formation of drops and subsequent infall to the centre.

2.0. Let us now consider at what distension the mass, expanding adiabatically, would begin to liquefy. We have

$$T_p = T_s\left(\frac{\rho_p}{\rho_s}\right)^{\gamma-1} \quad . \quad . \quad . \quad . \quad (1)$$

if the matter came from moderate depth in the sun. With $T_s = 10^7$ degrees, $\rho_s = 1$ gm./cm.^{3}, $T_p = 3 \times 10^3$ degrees, we have

$$\rho_p = \rho_s/(3 \times 10^3)^{\frac{1}{\gamma-1}}$$
$$= 6 \times 10^{-6} \text{ gm./cm.}^3 \text{ for } \gamma = \tfrac{5}{3}$$

or

$$= 2 \times 10^{-9} \text{ gm./cm.}^3 \text{ for } \gamma = \tfrac{7}{5}.$$

All the planets would liquefy at about the same density if made of the same material. The corresponding radii for some of the planets are as follows, in kilometres.

	Jupiter.	Saturn.	Earth.	Uranus.	Neptune.
$\gamma=\frac{5}{3}$	4×10^6	2×10^6	5×10^5	$1{\cdot}2\times10^6$	$1{\cdot}3\times10^6$
$\gamma=\frac{7}{5}$	6×10^8	3×10^8	8×10^6	$1{\cdot}9\times10^7$	$2{\cdot}0\times10^7$

Some comparable satellite distances are

J IV $1{\cdot}9 \times 10^6$ Titan $1{\cdot}2 \times 10^6$ Moon $3{\cdot}8 \times 10^5$ Oberon $5{\cdot}9 \times 10^5$
J VI 11 $\times 10^6$ Iapetus $3{\cdot}6 \times 10^6$ Neptune's satellite $3{\cdot}5 \times 10^5$
J IX 24×10^6 Phœbe 13×10^6

For superficial matter

$$T_o = (500)^{-\frac{1}{4}}T_s\,; \qquad \rho_o = (500)^{-\frac{3}{4}}\rho_s \quad . \quad . \quad (2)$$

nearly, and

$$\rho_p = \rho_o(T_p/T_o)^{\frac{1}{\gamma-1}}$$
$$= 500^{-\frac{3\gamma-4}{4(\gamma-1)}}\rho_s(T_p/T_s)^{\frac{1}{\gamma-1}} \quad . \quad . \quad . \quad (3)$$

We have numerically

γ	$\frac{5}{3}$	$\frac{7}{5}$	$\frac{4}{3}$
$\frac{3\gamma-4}{4(\gamma-1)}$	$\frac{3}{8}$	$\frac{1}{8}$	0
$500^{-\frac{3\gamma-4}{4(\gamma-1)}}$	$\frac{1}{10}$	$\frac{1}{2}$	1

The critical densities for bodies of the same mass, for $\gamma > \frac{4}{3}$, are less, and the critical radii therefore greater, than on the former hypothesis ; but the factor introduced into the radius is only 2 for $\gamma = \frac{5}{3}$ and 1·2 for $\gamma = \frac{7}{5}$. Lighter material, or matter with a smaller value of γ, or with a lower boiling-point, would not liquefy till it had attained a much greater radius. It is suggestive that the critical radii just given are of the same order of magnitude in each case as the distance of the most remote great satellite, though in the case of the moon the agreement is obviously accidental, since its distance must in any case have changed since formation. The more distant satellites may be formed of more volatile matter.

2.1. The velocity of radial expansion would be comparable with

the velocity of sound at the actual temperature, which is of the order of some kilometres a second. Jupiter would reach the critical radius for $\gamma = \frac{5}{3}$ in a week or so, and the earth in a day.

2.2. A planet liquefying in this way would have to condense through the droplets falling in towards the centre. Before they could aggregate they would have to radiate away not only both their original heat and the latent heat, but also the gravitational energy of condensation. The rate of radiation is $4\pi\sigma a^2 T^4$, where σ is Stefan's constant, a the radius, and T the surface temperature, here effectively the melting-point. The rate of loss of heat per unit mass is therefore $3\sigma T^4/\rho a$. Taking $\sigma(1^\circ)^4 = 5 \times 10^{-5}$ erg./gm. sec., $T = 3000^\circ$, $\rho = 6 \times 10^{-6}$ gm./cm.^{3}, $a = 4 \times 10^{11}$ cm., this is 5×10^3 ergs./gm. sec. or 10^{-4} cal./gm. sec. A reasonable initial store of heat would maintain this for 10^8 seconds, or a few years. The loss of gravitational energy per unit mass in condensing from radius a to $\frac{1}{2}a$ is of order fm/a, which is, for Jupiter at a radius 4×10^{11} cm., equal to 3×10^{11} ergs./gm. This also would take about 10^8 seconds to be lost by radiation. Thus after the initial rapid adiabatic expansion had led to liquefaction in drops near the outside, the further changes would be comparatively slow. The kinetic energy acquired in the infall of the drops would be mainly reconverted into heat by friction against the gas and by impact on the core, so that the accumulation at the centre would take place at such a rate as to keep pace with the loss of heat by radiation.

For the earth, at the same critical density, the rate of loss of heat by radiation per unit mass would be greater, and the initial store of heat about the same, so that the initial heat would be more quickly exhausted. The condensational energy per unit mass would be less in the same ratio as m/a, and would last for $\frac{1}{330}$ of the time found for Jupiter, that is, about a month. It appears that the condensation of all the planets would be in rather an advanced stage before their first perihelion passage.

3.0. On the tidal theory satellites have been supposed to be formed by the tidal disruption of their primaries at their first perihelion passage. This theory had the recommendation that it appeared to account qualitatively for the rotations of the planets. But it now appears that it is quantitatively inadequate for this purpose,* while the theory of material collision gives a quantitative explanation of the rotation of the planets without reference to the satellites. The origin of satellites therefore becomes a separate problem.

3.1. There is a special difficulty in the formation of satellites by tidal disruption. I showed in 1918 that there is a definite lower limit to the size of a body that can hold together in the gaseous state; a solid body with a density of 1 gm./cm^3 and a radius under 3400 km., about the dimensions of the largest satellite of Jupiter, would have insufficient gravitational attraction to hold itself together if it were volatilized.† Hardly any satellites, therefore, can have been originally gaseous spheres isolated in a vacuum. Now if the primary was still

* *M.N.*, **89**, 636–641, 1929.
† *The Earth*, 1929, p. 28.

gaseous when broken up tidally, the resulting filament would be under lower pressure and would expand farther, and there is no possibility of enough gravitational attraction to control the inherent tendency to spread into space. If the primary was liquid, the disruption would take place in a different manner, investigated by Jeans, and the satellites would be comparable in size with the primary. In neither case could tidal disruption of the planets give satellites of the actual small diameters. I have previously suggested a means of enabling at any rate a part of the mass to be retained, but it seems desirable now to consider other possibilities.

3.2. The result that the primitive planets would expand to radii comparable with the distances of their larger satellites suggests that the satellites may have been formed by internal condensation. We know that liquefaction is actually possible when the masses formed are small: I refer to raindrops. If the actual density of a constituent of a gas reaches the density of saturated vapour at the actual temperature, liquefaction will take place by condensation on small nuclei. Gravitation is not an important factor. But this process is not applicable here. A theory must account not only for the existence of the satellites, but also for their motions. A satellite formed by internal condensation would have to revolve about the centre in the same time as the local medium, and hence the medium locally would have to have a rotation comparable with that of a satellite at the same distance. Also the local density is supposed to be of the same order as the mean density of the planet; hence we have an estimate of the angular momentum of rotation of the planet, which must be comparable with that of a Laplacian nebula on the verge of disruption. The surviving angular momentum of the planetary subsystems is very much smaller, so that internal condensation will not explain the facts.

4.0. The collision theory suggests an explanation of satellites, though the details are not clear. In place of the roughly cylindrical filament of the tidal theory, the collision theory implies that the matter emerging from the sun formed a ribbon. If its shape was not affected by the mutual gravitation of its parts, internal friction, and adiabatic expansion, it would have a width comparable with the radius of the sun, an initial thickness about $\frac{1}{500}$ of this, and it would stretch from the sun to the star. Its initial density would be comparable with the mean density of the sun. An elongated ribbon of this type would certainly be gravitationally unstable.* Judging by Jeans's results for the instability of nearly homogeneous masses in other cases, instability is likely to arise when one dimension is about three times another. But a ribbon-like mass would tend to oscillate about the circular form, owing to the mutual gravitation of its parts, and friction would tend to damp this

* I do not think now that Jeans's discussion in *Problems of Cosmogony*, § 160, is applicable to either a filament or a ribbon. The type of disturbance that he considers is one-dimensional and involves no inward or outward motion from the central axis of the filament. But if we are to explain how a filament can break up into detached masses, motion at right angles to the axis is essential to the problem, and the boundary conditions at the sides of the filament, and not merely at the ends, must appear in the statement of it.

oscillation down and convert the ribbon into a circular filament. The period of such an oscillation would be of the order of $2\pi/(fp)^{\frac{1}{2}}$, which is about six hours. But the front of the filament was advancing at a rate of about 400 km./sec., so that when the transverse oscillation had proceeded through a quarter period the length of the ribbon was 2×10^6 km., or three times the radius of the sun. Conditions for longitudinal instability were therefore reached already. Longitudinal instability is partly hindered by the shearing motion, which would tend to displace the regions of thickening forward on one side, and therefore to average out the thickness again, as has been noticed in the problem of the streaming liquid heated below. But internal friction would tend to annul the shearing motion quickly and convert it into rotation. It seems certain in any case that the ribbon would be unstable and would break up into pieces within at most a few hours of emergence. It would be tempting to attribute the formation of the planets to this and that of the satellites to the transverse instability. But we must recall that the plane of the ribbon was at a considerable angle, perhaps nearly perpendicular to the plane of the relative motion of the sun and star. Fragments produced by transverse waves would move in orbits perpendicular to the plane of the ecliptic. This is, I think, the first hint of an explanation of the orbits of the satellites of Uranus and Neptune. The orbits of satellites of the other planets might have been brought into approximate agreement with the ecliptic and the equators of their primaries by the combined action of solar and equatorial attraction on them, in presence of a resisting medium. But it remains difficult to see why the nearer satellites should always be direct and only the most remote ones of Jupiter and Saturn retrograde.

4.1. It has been noticed that the planets, in expanding adiabatically till liquefaction began, would pass beyond the present orbits of their larger satellites: their more volatile constituents might pass beyond even the more distant satellites. The gaseous envelope might have had the effect of preventing the escape of matter from the smaller satellites till they had had time to liquefy. In this form of the theory the need for the matter near the satellites to share their density and motion disappears.

5.0. The foregoing investigations have been exploratory rather than decisive. It appears fairly clear, however, that on the collision theory the matter swept off the sun would form a ribbon-like mass, which would be unstable in a complicated way, and would probably break up longitudinally into nuclei after a few hours. Transverse rupture might also occur, and offers a hint concerning the origin of the anomalous motions of the satellites of Uranus and Neptune. It seems probable from other considerations that the satellites were formed at nearly the same time as the planets and not at a later date.

The planetary nuclei expanded adiabatically when formed, and liquefied when their density was about 6×10^{-6} gm./cm.^{3}. This took about a day for the earth and a week for Jupiter. The subsequent collection of the drops into liquid planets was controlled by radiation, and probably took a few years for Jupiter and some days for the earth.

Integral Theorems on the Equilibrium of a Star. By E. A. Milne.

1. It is of some interest in the theory of stellar equilibrium to develop theorems which are true for any star, independent of special assumptions as to the composition of the star, state (gas or liquid), constancy of molecular weight if a gas, distribution of energy generation, etc. Certain minimal theorems of this character have been formulated by Professor Eddington * under varying degrees of generality. It is the object of the present paper to state and prove a general theorem concerning certain integrals taken through the whole star, and to apply it in a number of particular cases. Amongst other results we arrive at (1) a formula for the "weighting" of $1-\beta$ through a star composed of perfect gas, as given by Eddington's quartic equation; (2) a lower limit to the central pressure in any star; (3) a concise proof of Emden's elegant theorem concerning the gravitational energy of a polytrope; (4) an analogous theorem, apparently not previously given, for the mean value of gravity (taken with regard to mass) through a polytrope. It is shown to be probable that no further integral theorems for polytropes remain to be discovered. The general formulation of Lane's law is given.

2. *Theorem.*—Let $M(r)$ be the mass contained inside radius r, G the constant of gravitation, P the total pressure at radius r (sum of radiation-pressure and gas-pressure). Let r_1 be the radius of the star, and let ν be any real number less than 4.

Then for any non-rotating star in mechanical equilibrium the following result holds:—

$$\int_0^{r_1} \frac{M(r)dM(r)}{r^\nu} = \frac{4\pi(4-\nu)}{G}\int_0^{r_1} P r^{3-\nu} dr \quad . \quad . \quad (1)$$

The theorem is true whatever the state of the star (gas, perfect or imperfect, or liquid), whatever the ionisation or chemical composition (however inhomogeneous), and whatever the distribution of sources of energy. It depends only on the equation of hydrostatic equilibrium.

Proof.—The equation of hydrostatic equilibrium is

$$\frac{dP}{dr} = -\frac{GM(r)}{r^2}\rho \quad . \quad . \quad . \quad . \quad (2)$$

where ρ is the density. The left-hand side of (1) is simply

$$\int_0^{r_1} \frac{M(r)}{r^\nu} 4\pi\rho r^2 dr,$$

and substituting in this from (2) for $\rho M(r)$ we have

$$\begin{aligned}\int_0^{r_1} \frac{M(r)dM(r)}{r^\nu} &= -\frac{4\pi}{G}\int_0^{r_1} r^{4-\nu}\frac{dP}{dr}dr \\ &= -\frac{4\pi}{G}\left[\left[P r^{4-\nu}\right]_0^{r_1} - (4-\nu)\int_0^{r_1} P r^{3-\nu} dr\right].\end{aligned}$$

* *Internal Constitution of the Stars*, pp. 90–94.

The value of P at the boundary may be taken as zero. Strictly it has a small non-zero value given by the Schwarzschild boundary temperature, but this is negligible compared with the values of P occurring in the other term on the right-hand side. Thus the integrated part vanishes at both limits and we arrive at result (1).

3. *Particular Cases.*—Three particular cases prove to be of interest, namely $\nu = 0, \nu = 1$, and $\nu = 2$. They give, if M is the total mass,

$$M^2 = \frac{32\pi}{G}\int_0^{r_1} Pr^3 dr \quad . \quad . \quad . \quad . \quad (3)$$

$$\int_0^{r_1}\frac{GM(r)dM(r)}{r} = 12\pi\int_0^{r_1} Pr^2 dr \quad . \quad . \quad . \quad (4)$$

$$\int_0^{r_1}\frac{GM(r)dM(r)}{r^2} = 8\pi\int_0^{r_1} Prdr \quad . \quad . \quad . \quad (5)$$

The first of these is an integral expression for the square of the mass which does not seem to have been previously given. Physical interpretations of the second and third relations are easily found. The left-hand side of (4) is simply Ω, the (negative) gravitational energy ; this is easily seen by bringing up successive spherical shells from infinity under the attraction of the interior mass. The left-hand side of (5) is simply $M\bar{g}$ where $\bar{g}$, the mean value of gravity through the star, is defined by

$$\bar{g} = \frac{1}{M}\int g dM(r) = \frac{1}{M}\int\frac{GM(r)dM(r)}{r^2}.$$

Putting dv for the element of volume $4\pi r^2 dr$, we see that (4) may be written

$$\Omega = 3\int_0^{r_1} P dv \quad . \quad . \quad . \quad . \quad (6)$$

or again

$$\Omega = 3\int_0^{r_1}\frac{P}{\rho}dM(r) \quad . \quad . \quad . \quad (7)$$

The extreme simplicity of (6) may be noted. Stated in this general form it does not appear to have been previously given, though the transformations establishing it were used by Eddington in discussing the mean temperature of a star. I have employed (6) as the basis of a discussion of formulæ for the total energy of a star in an as yet unpublished contribution to *Handbuch der Astrophysik.*

Equation (5) may be written

$$\bar{g} = \frac{8\pi}{M}\int_0^{r_1} Prdr \quad . \quad . \quad . \quad . \quad (8)$$

4. We can now see how M, Ω, and $\bar{g}$ vary, (1) with the arbitrary constants which define the star, (2) with the general form of the density-distribution. On inserting

$$M(r) = \int \rho 4\pi r^2 dr \quad . \quad . \quad . \quad . \quad (9)$$

in the equation of equilibrium (2) we find

$$\frac{1}{r^2}\frac{d}{dr}\left(\frac{r^2}{\rho}\frac{dP}{dr}\right) = -4\pi G\rho \quad . \quad . \quad . \quad (10)$$

Put

$$P = P_c y, \quad \rho = \rho_c z \quad . \quad . \quad . \quad . \quad (11)$$

where P_c, ρ_c are the central values. Then

$$\frac{P_c}{4\pi G{\rho_c}^2}\frac{1}{r^2}\frac{d}{dr}\left(\frac{r^2}{z}\frac{dy}{dr}\right) = -z.$$

Now put

$$\frac{P_c}{4\pi G{\rho_c}^2}\frac{1}{r^2} = \frac{1}{x^2} \quad . \quad . \quad . \quad . \quad (12)$$

Then

$$\frac{1}{x^2}\frac{d}{dx}\left(\frac{x^2}{z}\frac{dy}{dx}\right) = -z \quad . \quad . \quad . \quad . \quad (13)$$

The differential equation (13) determines the form of the density-distribution when y is given as a function of z, say $y = f(z)$. We then select that solution for which, at $x = 0$, $z = 1$, and $dz/dx = 0$. Equation (13) is independent of the constants P_c, ρ_c. It defines a family of homologous stars once $f(z)$ is specified.

Substituting from (11) and (12) in (3), (4), and (5) we have

$$M^2 = \frac{2}{\pi G^3}\frac{{P_c}^3}{{\rho_c}^4}\int_0^{x_1} yx^3dx \quad . \quad . \quad . \quad (14)$$

$$\Omega = \frac{3{P_c}^{\frac{4}{3}}}{2\pi^{\frac{1}{3}}G^{\frac{2}{3}}{\rho_c}^3}\int_0^{x_1} yx^2dx \quad . \quad . \quad . \quad (15)$$

$$\bar{g} = \frac{2{P_c}^2}{GM{\rho_c}^2}\int_0^{x_1} yxdx \quad . \quad . \quad . \quad . \quad (16)$$

where $x = x_1$ is the first zero of $z(x)$ or $y(x)$. For completeness we add

$$r_1 = \frac{{P_c}^{\frac{1}{3}}}{2\pi^{\frac{1}{3}}G^{\frac{1}{2}}\rho_c}x_1 \quad . \quad . \quad . \quad . \quad (17)$$

Equations (15) and (16) can then be put in the form

$$\Omega = \frac{3}{8}\frac{GM^2}{r_1}\frac{x_1\int_0^{x_1} yx^2dx}{\int_0^{x_1} yx^3dx} \quad . \quad . \quad . \quad (18)$$

$$\bar{g} = \frac{1}{4}\frac{GM}{{r_1}^2}\frac{{x_1}^2\int_0^{x_1} yxdx}{\int_0^{x_1} yx^3dx} \quad . \quad . \quad . \quad (19)$$

The factors involving integrals are of course simply numerical coefficients depending on the form of the density-distribution.

The integral $\int_0^{x_1} yx^3dx$ can easily be evaluated without further quadrature once the differential equation (13) has been solved. For (13) may be written

$$\left(\frac{x^2}{z}\frac{dy}{dx}\right)\frac{d}{dx}\left(\frac{x^2}{z}\frac{dy}{dx}\right) = -x^4\frac{dy}{dx}.$$

Integrating and using $dy/dx = 0$ at $x = 0$, we have

$$\frac{1}{2}\left(\frac{x^2}{z}\frac{dy}{dx}\right)^2 = -\int_0^x x^4\frac{dy}{dx}$$
$$= -yx^4 + 4\int_0^x yx^3dx.$$

Hence

$$\int_0^{x_1} yx^3dx = \frac{1}{8}\left(\frac{x^2}{z}\frac{dy}{dx}\right)^2_{x=x_1} \quad . \quad . \quad . \quad (20)$$

This shows that (14) is consistent with the usual mass integral obtained by substituting for ρ and r from (11) and (12) in (9).

5. *An Inequality for* P_c.—The differential equation of hydrostatic equilibrium shows that P always increases inwards. Hence by (3)

$$M^2 < \frac{32\pi P_c}{G}\int_0^{r_1} r^3dr = \frac{8\pi P_c r_1^4}{G}.$$

Hence

$$P_c > \frac{GM^2}{8\pi r_1^4} \quad . \quad . \quad . \quad . \quad (21)$$

This sets a lower limit to the value of the total pressure at the centre of any star. Eddington has shown previously that, if we consider only stars for which the *density* increases inwards, then $P_c > 3GM^2/8\pi r_1^4$. For a sufficient concentration of energy generation to the centre, the density may, however, *decrease* inwards, and it is therefore of interest to have the more general inequality (21).

5*a*. *Inequalities for* Ω *and* $\bar{g}$.—Since $x < x_1$ throughout the star, we have

$$\int_0^{x_1} yx^3dx < x_1\int_0^{x_1} yx^2dx$$

and

$$\int_0^{x_1} yx^3dx < x_1^2\int_0^{x_1} yxdx.$$

Inserting these in (18) and (19) we have

$$\Omega > \frac{3}{8}\frac{GM^2}{r_1}$$
$$\bar{g} > \frac{1}{4}\frac{GM}{r_1^2}.$$

Again, since the pressure necessarily increases through the star, we have from (4) and (5)

$$\Omega < 4\pi P_c r_1^3$$

$$\bar{g} < \frac{4\pi P_c r_1^2}{M}.$$

Combining the above inequalities we have

$$4\pi P_c r_1^3 > \Omega > \frac{3}{8}\frac{GM^2}{r_1} \quad . \quad . \quad . \quad (21a)$$

$$\frac{4\pi P_c r_1^2}{M} > \bar{g} > \frac{1}{4}\frac{GM}{r_1^2} \quad . \quad . \quad . \quad . \quad (21b)$$

These hold for any star.

6. *Application to Perfect Gas.*—Equation (14) may be regarded as the generalisation of Eddington's quartic equation for "$1-\beta$." When "$1-\beta$" is not constant through the star we still have, provided the centre is composed of perfect gas, the relations

$$(1-\beta_c)P_c = \tfrac{1}{3}aT_c^4$$
$$\beta_c P_c = (R/\mu_c)\rho_c T_c,$$

where T_c is the central temperature and μ_c is the molecular weight at the centre. Eliminating T_c we have

$$\frac{P_c^3}{\rho_c^4} = \frac{1-\beta_c}{\beta_c^4}\left(\frac{R}{\mu_c}\right)^4 \frac{1}{\frac{1}{3}a},$$

so that from (14)

$$M^2 = \frac{6}{\pi a G^3}\frac{1-\beta_c}{\beta_c^4}\left(\frac{R}{\mu_c}\right)^4 \int_0^{x_1} yx^3 dx \quad . \quad . \quad (22)$$

The original integral (3) can also be used to exhibit M^2 as an integral of $(1-\beta)/\beta^4\mu^4$ through the star. For a star composed of perfect gas, we have as above

$$\frac{P^3}{\rho^4} = \frac{1-\beta}{\beta^4}\left(\frac{R}{\mu}\right)^4 \frac{1}{\frac{1}{3}a},$$

whence by (3)

$$M^2 = \frac{96\pi}{aG}\int_0^{r_1}\frac{1-\beta}{\beta^4}\left(\frac{R}{\mu}\right)^4 \frac{\rho^4}{P^2} r^3 dr \quad . \quad . \quad . \quad (23)$$

This tells us the proper "weighting" of $(1-\beta)/\beta^4\mu^4$ through the star to give M^2. The weighting factor $(\rho^4/P^2)r^3dr$ is independent of the arbitrary constants ρ_c and P_c which define the star. For it may be written

$$\frac{(\rho_c z)^4}{(P_c y)^2}\left(\frac{P_c}{4\pi G\rho_c^2}\right)^2 x^3 dx,$$

or

$$\frac{1}{16\pi^2 G^2}\frac{z^4}{y^2}x^3 dx.$$

The weighting factor thus depends only on the *form* of the density-distribution.

For an element of mass $dM(r)$ the weighting factor is proportional to $(\rho^3 r/P^2)dM(r)$. For the polytrope $n = 3$, for which $1 - \beta$ and μ are constant, $z^4 = y^3$, and the integral occurring in (23) reduces to $\int yx^3 dx$ which we have evaluated above.

7. *The General Proof of Lane's Law.*—From (14) and (17) we have, if s is any number,

$$\frac{M^2}{r_1^s} = \frac{2^{s+1}\pi^{\frac{1}{2}s-1}}{G^{3-\frac{1}{2}s}\rho_c^{4-s}} P_c^{3-\frac{1}{2}s} \cdot \frac{1}{x_1^s}\int_0^{x_1} yx^3 dx \quad . \quad . \quad (24)$$

The cases $s = 2$, $s = 4$, $s = 6$ give respectively

$$\frac{M^2}{r_1^2} = \frac{2^3}{G^2}\frac{P_c^2}{\rho_c^2}\frac{1}{x_1^2}\int_0^{x_1} yx^3 dx \quad . \quad . \quad . \quad (25)$$

$$\frac{M^2}{r_1^4} = \frac{2^5\pi}{G} P_c \frac{1}{x_1^4}\int_0^{x_1} yx^3 dx \quad . \quad . \quad . \quad (26)$$

$$\frac{M^2}{r_1^6} = 2^7\pi^2\rho_c^2\frac{1}{x_1^6}\int_0^{x_1} yx^3 dx \quad . \quad . \quad . \quad (27)$$

If P_c consists almost entirely of gas-pressure, then $P_c/\rho_c \propto T_c$, and (25) gives

$$r_1 T_c \propto M \, . \quad . \quad . \quad . \quad . \quad (28)$$

for stars of given density-distribution, the constant of proportionality being independent of the density. This is Lane's law for stars composed of perfect gas of mass so low that radiation-pressure is negligible.

If P consists almost entirely of radiation-pressure, then $P_c \propto T_1^4$, and (26) gives

$$r_1 T_c \propto M^{\frac{1}{2}} \, . \quad . \quad . \quad . \quad . \quad (29)$$

for stars of given density-distribution, the constant of proportionality being as before independent of the density. This is Lane's law for massive stars of perfect gas.

It is by way of a mathematical accident that Lane's law, originally enunciated for the case when radiation-pressure is neglected, should also be true when radiation-pressure is taken into account. Were radiation-pressure proportional to any other power of temperature than the fourth, Lane's law would not hold when radiation-pressure is taken into account. The approximations (28) and (29) suggest that Lane's law is true in general when radiation-pressure is taken into account, and this is readily verified. We have in fact, for perfect gas at the centre of the star,

$$P_c = \tfrac{1}{3}aT_c^4 + (R/\mu_c)\rho_c T_c,$$

whence

$$P_c r_1^4 = \tfrac{1}{3}a(T_c r_1)^4 + (R/\mu_c)(\rho_c r_1^3)(T_c r_1) \quad . \quad . \quad (30)$$

Substituting for $P_c r_1^4$ from (26) and for $\rho_c r_1^3$ from (27), we have

$$\frac{GM^2 x_1^4}{2^5\pi\int_0^{x_1} yx^3 dx} = \tfrac{1}{3}a(T_c r_1)^4 + \frac{Mx_1^3(R/\mu_c)}{2^{\frac{1}{2}}\pi\left[\int_0^{x_1} yx^3 dx\right]^{\frac{1}{2}}}(T_c r_1) \quad . \quad (31)$$

This is the most general expression of Lane's law. It shows that the product $T_c r_1$ is the root of a quartic equation in which the coefficients depend on the total mass and on the form of the density-distribution, but are independent of the density.

8. *The Total Gravitational Energy of a Polytropic Star.*—It is proposed to show how equation (7) leads to a concise proof of Emden's formula for the gravitational energy of a polytrope. For a polytrope of class n we have *

$$P \propto \rho^{1+\frac{1}{n}},$$

and accordingly

$$\frac{dP}{\rho} = (n+1)d\left(\frac{P}{\rho}\right) \quad . \quad . \quad . \quad . \quad (32)$$

If we write the equation of equilibrium in the form

$$\frac{1}{\rho}\frac{dP}{dr} = \frac{dV}{dr} \quad . \quad . \quad . \quad . \quad (33)$$

where V is the gravitational potential, insert (32) and integrate, we obtain

$$(n+1)\frac{P}{\rho} = V - V_1 \, . \quad . \quad . \quad . \quad (34)$$

Here V_1 is the boundary value GM/r_1. Equation (34) may be considered as characteristic of polytropic equilibrium.

By elementary potential theory, we have

$$\Omega = \tfrac{1}{2}\int V dM(r) \quad . \quad . \quad . \quad . \quad (35)$$

or by (34)

$$\Omega = \tfrac{1}{2}\int\left[V_1 + (n+1)\frac{P}{\rho}\right]dM(r) \, . \quad . \quad . \quad (36)$$

Hence, by our formula (7) for $\int (P/\rho)dM(r)$,

$$\Omega = \tfrac{1}{2}V_1 M + \frac{n+1}{6}\Omega,$$

or

$$\Omega = \frac{3}{5-n}\frac{GM^2}{r_1} \, . \quad . \quad . \quad . \quad (37)$$

This is Emden's result.

It should be remarked that the equivalence of the two formulæ we have used for Ω,

$$\Omega = \int\frac{GM(r)dM(r)}{r} = \tfrac{1}{2}\int V dM(r),$$

is easily verified by integration by parts, using $dV/dr = -GM(r)/r^2$. It does not depend upon the equation of equilibrium.

* We are actually using a wider definition of a polytrope than Emden's, since P is the total pressure. Our analysis is carefully arranged to involve no mention of temperature in treating of polytropes.

9. *The Mean Value of Gravity through a Polytropic Star.*—A similar method of proof allows us to obtain a formula for the mean value of g through a polytropic star without further quadrature once the equation of polytropic equilibrium has been solved.

We require to evaluate $\int_0^{r_1} P r dr$. We have

$$4\pi \int_0^{r_1} P r dr = \int_0^{r_1} \frac{P}{\rho r} dM(r) \quad . \quad . \quad . \quad (38)$$

or by (34), for a polytropic star,

$$4\pi(n+1) \int_0^{r_1} P r dr = \int_0^{r_1} \frac{V - V_1}{r} dM(r) \quad . \quad . \quad (39)$$

The integral on the right can be reduced to a sum of integrated terms, together with a multiple of the original integral $\int P r dr$, the transformations involving only the equation of equilibrium. We have, on integrating by parts,

$$\int_0^{r_1} \frac{V - V_1}{r} dM(r) = \int M(r) \left[\frac{1}{r} \frac{dV}{dr} - \frac{V - V_1}{r^2} \right] dr \quad . \quad (40)$$

the integrated part vanishing at both limits. Taking in turn the three integrals on the right-hand side of (40), we have first

$$-\int_0^{r_1} \frac{M(r)}{r} \frac{dV}{dr} = G \int_0^{r_1} \frac{[M(r)]^2}{r^3} dr = -\frac{GM^2}{2r_1^2} + G \int_0^{r_1} \frac{M(r) dM(r)}{r^2} dr$$

$$= -\frac{GM^2}{2r_1^2} + 8\pi \int P r dr \quad . \quad . \quad . \quad . \quad (41)$$

on using our theorem in the form (5); secondly

$$\int_0^{r_1} \frac{M(r)}{r^2} V dr = -\frac{1}{G} \int_0^{r_1} V \frac{dV}{dr} dr = \frac{1}{2G} [V_c^2 - V_1^2] : \quad . \quad (42)$$

thirdly

$$-V_1 \int_0^{r_1} \frac{M(r)}{r^2} dr = \frac{V_1}{G} \int_0^{r_1} \frac{dV}{dr} dr = \frac{V_1}{G} [V_1 - V_c] \quad . \quad (43)$$

Combining our results, we have

$$4\pi(n+1) \int_0^{r_1} P r dr = -\frac{GM^2}{2r_1^2} + 8\pi \int_0^{r_1} P r dr + \frac{1}{2G} [V_c^2 - V_1^2] + \frac{V_1}{G} [V_1 - V_c],$$

or

$$4\pi(n-1) \int_0^{r_1} P r dr = \frac{1}{2G} [V_c^2 - 2V_1 V_c],$$

since $V_1 = GM/r_1$; or again

$$4\pi\int_0^{r_1} Prdr = \frac{1}{2(n-1)G}[(V_c - V_1)^2 - V_1^2] \quad . \quad (44)$$

Using again the polytropic relation (34), this becomes

$$4\pi\int Prdr = \frac{1}{2(n-1)G}\left[(n+1)^2\frac{P_c^2}{\rho_c^2} - \frac{G^2M^2}{r_1^2}\right] \quad . \quad (45)$$

Hence by (8)

$$\bar{g} = \frac{1}{n-1}\left[\frac{(n+1)^2P_c^2}{GM\rho_c^2} - \frac{GM}{r_1^2}\right] \quad . \quad . \quad . \quad (46)$$

To put this in a form suitable for computation, we substitute for P_c^2/ρ_c^2 from (25), obtaining

$$\bar{g} = \frac{1}{n-1}\frac{GM}{r_1^2}\left[\frac{(n+1)^2}{8}\frac{x_1^2}{\int_0^{x_1} yx^3dx} - 1\right] \quad . \quad . \quad (47)$$

or by (20)

$$\bar{g} = \frac{1}{n-1}\frac{GM}{r_1^2}\left[\frac{(n+1)^2}{\left[\frac{x}{z}\frac{dy}{dx}\right]_1^2} - 1\right] \quad . \quad . \quad . \quad (48)$$

To use Emden's tables we must introduce Emden's variables. Write

$$y = \theta^{n+1}, \quad z = \theta^n,$$

$$\frac{n+1}{x^2} = \frac{1}{\xi^2};$$

then the differential equation (13) reduces to Emden's equation

$$\frac{1}{\xi^2}\frac{d}{d\xi}\left[\xi^2\frac{d\theta}{d\xi}\right] = -\theta^n \quad . \quad . \quad . \quad . \quad (49)$$

Equation (48) then becomes, if ξ_1 is the first zero of $\theta(\xi)$,

$$\bar{g} = \frac{1}{n-1}\frac{GM}{r_1^2}\left[\frac{1}{\xi_1^2\left(\frac{d\theta}{d\xi}\right)_1^2} - 1\right] \quad . \quad . \quad (50)$$

For the polytrope $n = 3$ (Eddington's model), $\xi_1 = 6{\cdot}9011$ and $(d\theta/d\xi)_1 = -0{\cdot}04231$; hence

$$\frac{1}{n-1}\left[\frac{1}{\xi_1^2\left(\frac{d\theta}{d\xi}\right)_1^2} - 1\right] = 5{\cdot}865.$$

Thus for a star built on Eddington's model, the mean value of gravity per gram of material is 5·865 times the surface gravity. For the sun, for which surface gravity is $2{\cdot}74 \times 10^4$ cm. sec.$^{-2}$, the mean value of g through the interior is accordingly $1{\cdot}607 \times 10^5$, or 164 times terrestrial gravity.

10. *Proof of the non-Existence of other Integrable Cases for Polytropic Equilibrium.*—We have seen that $\int \mathrm{P}r^3 dr$ always represents the square of the mass. We have seen further that in polytropic equilibrium $\int \mathrm{P}r^2 dr$ and $\int Prdr$, which represent Ω and $\bar{g}$, can also be evaluated without further quadrature once the equation of polytropic equilibrium has been solved. The question arises whether $\int \mathrm{P}r^{3-\nu}dr$ can be evaluated for polytropes in any other cases besides $\nu = 0$, $\nu = 1$, and $\nu = 2$. The following investigation shows why these special cases are integrable, and practically amounts to a proof that the integration cannot be effected without recourse to further quadratures in any other cases.

In terms of Emden's variables θ and ξ we have to investigate the value of

$$\int_0^{\xi_1} \theta^{n+1}\xi^s d\xi,$$

θ and ξ being connected by (49), and ξ_1 being the first zero of $\theta(\xi)$. We can reduce this integral in two ways as follows. First,

$$\begin{aligned}\int_0^{\xi_1}\theta^{n+1}\xi^s d\xi &= -\int_0^{\xi_1}\theta\xi^{s-2}\frac{d}{d\xi}\Big(\xi^2\frac{d\theta}{d\xi}\Big)d\xi \\ &= -\Big[\theta\frac{d\theta}{d\xi}\xi^s\Big]_0^{\xi_1} + \int_0^{\xi_1}\xi^2\frac{d\theta}{d\xi}\Big[(s-2)\xi^{s-3}\theta + \xi^{s-2}\frac{d\theta}{d\xi}\Big]d\xi \\ &= (s-2)\int_0^{\xi_1}\xi^{s-1}\theta\frac{d\theta}{d\xi} + \int_0^{\xi_1}\xi^s\Big(\frac{d\theta}{d\xi}\Big)^2 d\xi \quad . \quad . \quad (51)\end{aligned}$$

Secondly,

$$\begin{aligned}\int_0^{\xi_1}\theta^{n+1}\xi^s d\xi &= \Big[\frac{\xi^{s+1}}{s+1}\theta^{n+1}\Big]_0^{\xi_1} - \frac{n+1}{s+1}\int_0^{\xi_1}\xi^{s+1}\theta^2\frac{d\theta}{d\xi}d\xi \\ &= \frac{n+1}{s+1}\int_0^{\xi_1}\xi^{s-1}\frac{d\theta}{d\xi}\frac{d}{d\xi}\Big(\xi^2\frac{d\theta}{d\xi}\Big)d\xi \\ &= \frac{n+1}{s+1}\int_0^{\xi_1}\frac{1}{2}\xi^{s-3}\frac{d}{d\xi}\Big[\Big(\xi^2\frac{d\theta}{d\xi}\Big)^2\Big]d\xi \\ &= \frac{n+1}{2(s+1)}\Big[\xi^{s-3}\Big(\xi^2\frac{d\theta}{d\xi}\Big)^2\Big]_0^{\xi_1} - \frac{(n+1)(s-3)}{2(s+1)}\int_0^{\xi_1}\xi^s\Big(\frac{d\theta}{d\xi}\Big)^2 d\xi \quad (52)\end{aligned}$$

Eliminating $\int_0^{\xi_1}\xi^s(d\theta/d\xi)^2 d\xi$ between (51) and (52) we have, on solving for the integral we are investigating,

$$\begin{aligned}[n(s-3)+3s-1]\int_0^{\xi_1}\theta^{n+1}\xi^s d\xi &= (n+1)\xi_1{}^{s+1}\Big(\frac{d\theta}{d\xi}\Big)_1^2 \\ &\quad + (n+1)(s-3)(s-2)\int_0^{\xi_1}\xi^{s-1}\theta\frac{d\theta}{d\xi}d\xi \quad (53)\end{aligned}$$

When $s = 2$ or $s = 3$ the term involving the integral on the right-hand side vanishes. When $s = 1$ the integral on the right-hand side is $[\frac{1}{2}\theta^2]_0^{\xi_1} = -\frac{1}{2}$. In each of these cases we have an evaluation of the integral on the left-hand side. For any other value of s there seems to be no way of evaluating $\int \xi^{s-1}\theta d\theta$, and accordingly only for $s = 1$, 2, or 3 does (53) suffice to evaluate $\int \theta^{n+1}\xi^s d\xi$. The results in the integrable cases are

$$s = 3, \quad \int_0^{\xi_1} \theta^{n+1}\xi^3 d\xi = \frac{n+1}{8}\xi_1^4\left(\frac{d\theta}{d\xi}\right)_1^2 \quad . \quad . \quad . \quad (54)$$

$$s = 2, \quad \int_0^{\xi_1} \theta^{n+1}\xi^2 d\xi = \frac{n+1}{5-n}\xi_1^3\left(\frac{d\theta}{d\xi_1}\right)^2 \quad . \quad . \quad . \quad (55)$$

$$s = 1, \quad \int_0^{\xi_1} \theta^{n+1}\xi d\xi = \frac{n+1}{2(n-1)}\left[1 - \xi_1^2\left(\frac{d\theta}{d\xi}\right)_1^2\right] \quad . \quad (56)$$

In terms of the variables y, z, x the results are

$$\int yx^3 dx = \frac{1}{8}\left(\frac{x^2}{z}\frac{dy}{dx}\right)_1^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

$$\int yx^2 dx = \frac{1}{5-n}x_1^3\left(\frac{1}{z}\frac{dy}{dx}\right)_1^2 \quad . \quad . \quad . \quad . \quad (58)$$

$$\int yx dx = \frac{1}{2(n-1)}\left[(n+1)^2 - x^2\left(\frac{1}{z}\frac{dy}{dx}\right)_1^2\right] \quad . \quad . \quad (59)$$

The first of these is identical with (20), as it must be. The second and third are only true for polytropes, since the polytropic index n is involved.

11. Equation (56) shows that

$$\xi_1 \left|\frac{d\theta}{d\xi}\right|_1 \lesseqgtr 1 \quad . \quad . \quad . \quad . \quad . \quad (60)$$

according as

$$n \lesseqgtr 1 \quad . \quad . \quad . \quad . \quad . \quad (61)$$

This is in agreement with Emden's tables. For $n = 1$, the value of $\xi_1 \mid d\theta/d\xi \mid_1$ is unity, as is easily verified from the known solution $\theta = (\sin \xi)/\xi$.

By combination of (25) and (20) we have

$$\frac{GM}{r_1} = \frac{P_c}{\rho_c}\left|\frac{x}{z}\frac{dy}{dx}\right|_1 \quad . \quad . \quad . \quad . \quad (62)$$

By (59) we have for a polytrope

$$\left|\frac{x}{z}\frac{dy}{dx}\right|_1 \lesseqgtr n+1 \quad . \quad . \quad . \quad . \quad (63)$$

according as $n \gtrless 1$. Hence, by (62)

$$\frac{P_c}{\rho_c} \gtrless \frac{1}{n+1} \frac{GM}{r_1} \quad . \quad . \quad . \quad . \quad (64)$$

according as $n \gtrless 1$. For a perfect gas we have $P_c/\rho_c = RT_c/\beta_c\mu_c$, and therefore under the same inequalities for n,

$$T_c \gtrless \frac{\beta_c\mu_c}{R} \frac{GM}{r_1} \frac{1}{n+1} . \quad . \quad . \quad . \quad (65)$$

The exact values of P_c/ρ_c for different polytropes are of course calculable directly from Emden's tables, but it is interesting to be able to establish this much without numerical quadrature.

12. *Summary.*—A general theorem, true for any star whatever its structure, involving integrals of pressure and mass through the star, is stated and proved. Particular cases of it yield expressions for the mass, the gravitational energy, Ω, and the mean value of gravity with respect to mass, $\bar{g}$, as integrals of the total pressure. A lower limit is deduced for the central pressure in any star, and upper and lower limits are deduced for Ω and $\bar{g}$. In applications to gaseous stars, Lane's law is stated and proved in its most general form, and the weighting of "$1 - \beta$" through a star to give the correct mass is obtained. Lastly, a number of theorems are proved for stars built on the polytropic model of Emden.

Harvard College Observatory :
1929 *September.*

LIST OF ADDITIONS

TO THE LIBRARY

OF THE SOCIETY

1928 JUNE 30 TO 1929 JUNE 30.

Serial Publications received in the regular course, and Reprints of Papers published in some of these Serials, are not included in this Annual List.

An asterisk (*) indicates that the work is an excerpt.

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† *See* Obituary Notice of T. W. Backhouse, *M.N.*, **81**, 255.

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PHOTOGRAPHS, Etc., PRESENTED TO THE SOCIETY.

Lt.-Col. John Day. A 2-inch Telescope by Tully, formerly belonging to Francis Baily.

Mr. J. G. Hawke. Framed Photograph of the Royal Astronomical Society's Rooms at Somerset House (1834–1874).

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